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EXHIBITING A VIEW OF THE

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IN THE

SCIENCES AND THE ARTS.

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PHILOSOPHICAL JOURNAL.

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ON Saturday, the 25th of June 1859, at 11.30 P.M. (Greenwich time) some men on the pier of Par, three miles West of Fowey, suddenly heard a sound approaching them from the open sea, like the rushing of a first-class steamer, and they thought such a steamer had actually mistaken her course, and would in a few seconds be wrecked at their feet. But what they thus, in the darkness of the night, took for a large steamer proved to be a tremendous current rushing into the harbour, breaking the hawsers by which the vessels were fastened, and dashing them against each other in a most fearful manner. The mouth of the harbour is forty yards wide, opening towards the S.E., and at low water of ordinary spring tides the sea retires from it to a distance of sixty fathoms, the shore then presenting a plain of sand extending nearly a mile from N. to S., and more than half a mile from E. to W. The ordinary neap tides rise in Par harbour 10 feet, and the ordinary spring tides 14. At the time mentioned it was neap tide, and one hour before high water, yet the mark left by the water along the beach of the harbour close to the Cornwall Railway, as seen the next morning, showed a rise of fifteen feet, which is one foot higher than the mark reached by ordinary spring tides. Had this happened at high water on a spring tide, some hundreds of tons of valuable copper ore would have been swept from the pier into the sea. Such was the violence of the current, that a schooner, which drew about 9 feet of water, and would barely float at neap tide, was borne along, dragging her deeply-imbedded anchor, until she was left on the mud in the harbour as high as she could have gone at spring tide, and it was necessary, a day or two afterwards, to take out a great part of her cargo before she could be removed from her strange position. Another schooner, after her hawsers had been snapped by the influx, and whilst drifting out with the retiring waters, let go her anchor with twenty fathoms of chain cable, but both anchor and vessel were carried out of the pier with great velocity to the distance of a furlong or two, when the current stayed, and the wind (which was south) drifted her on shore. By this influx and efflux, and the others which immediately followed, the ground at the mouth of the harbour was excavated to a depth of 4 or 5 feet beyond what had ever been previously known, and rocks were thereby exposed to view which had never been seen before. The interval between the commencement of two successive influxes was about fifteen minutes. This information I obtained on the spot from eye-witnesses.

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* Read before the Royal Geological Society of Cornwall on the 28th of October 1859.

There were similar agitations the same night at Penzance, and at all the piers in Mountsbay, as well as in the harbours of Falmouth, Fowey, and Plymouth. But the only place on the northern coast of Cornwall where it was observed was Bude-haven. There it occurred as early as noon, when the sea suddenly rose between 4 and 5 feet, which is just as high as it rose at Par and in Mountsbay. At Penzance and Mousehole it was noticed in the afternoon as well as at night. The time of its greatest observed violence in Penzance at the eastern end of the Esplanade, was the same as at Par and near the time of high water. It did not cease at Penzance until the middle of the following day. During its continuance it occasionally renewed its violence.

Thus was the sea on the Cornish coasts extraordinarily agitated from Saturday noon until Sunday noon, during which time a dreadful thunder storm, attended in some places with violent squalls of wind and heavy rains, was passing throughout the south of England from its western to its eastern extremity—beginning about noon on Saturday at the Landsend, and reaching London the following morning soon after 7 o'clock, when several persons were struck by the lightning. The barometer on this occasion, as it is almost always on such occasions, was at a minimum. At the Kew Observatory of the British Association on the 24th and three following days, the daily mean heights were 30·111, 29·934, 29·890 and 30·132—the wind on the 25th being moderate from about S.E. and S.S.E. The thermometer at the same observatory on the 25th and 26th was 72·2 and 77·4, the latter being the maximum of the year.

Another of these phenomena, more remarkable than any recorded since those on the days of the two great earthquakes of Lisbon, occurred on the coasts of Cornwall, Devon, Somerset, and Wales, on Tuesday the 4th of October 1859, occasioning no little alarm by rushing up tidal rivers several miles from their mouths. In describing it, I will begin with the Scilly Isles, then proceed eastward along the southern coast of Cornwall, and return westward by the Bristol Channel.

The following is the tide-gauger's report of what occurred at the pier of St Mary's, Scilly:—"At 7 A.M. there were 11 feet on the tide-gauge: it then fell to 9 feet, making no stop; it began to rise, and in 6 minutes after there were 14 feet 7 inches on the gauge; it made no stop, but returned back to its old mark with ebbing and flowing in a very disturbed state. William Tonkin, tide-gaugeman." For this I am indebted to Captain Williams, R.N., on the Admiralty Survey. About the time stated in the gauger's report, the sea in most of the islands was observed to rise above the high-water mark of the preceding tide, and, after retiring, to flow in again to the same height. Another efflux and influx immediately succeeded, but less extensive. This, and the state of the weather at Scilly, presently to be noticed, I learnt through the kindness of the President of this Society.

At Mousehole and Newlyn in Mountsbay, a similar agitation was observed between 6 and 7 the same morning, which continued for several hours, the interval between two successive influxes being about 15 minutes, and the greatest rise during one influx being between 5 and 6 feet. The boats at their moorings in Guavas "Lake," near Newlyn pier, veered with their bows to the current at every change in its direction. The agitation at Newlyn was observed as early as 2 or 3 in the morning, although it was then less considerable than between 6 and 7; hence probably it occurred unobserved in most parts of the bay long before sunrise.

At Penzance, a boatman left the Battery cove about 6 in the morning, perceiving nothing unusual in the water, but on his return half-an-hour afterwards he observed the sea rushing from the shore like a river, between the Battery rock and the Round rock, eddying and foaming in a most extraordinary manner. The agitation did not extend seaward beyond low-water line, and the current alternated from north to south, and from south to north, at intervals of a few minutes, always turning towards it the bows of the boats moored outside the cove. About this time, or before 7 o'clock, a current rushed into Penzance pier, rising about 4 feet, submerging the large buoys, floating vessels previously aground, breaking the moorings of a raft of timber, and carrying it rapidly out of the pier, until, the current being spent, the wind drifted it on shore eastward of the railway viaduct. This was followed by a great many similar influxes and effluxes of gradually diminishing violence. Soon after 10 A.M., when the commotion was much less than at its beginning, I observed by my watch, that the intervals between the commencements of two successive influxes, at the steps nearest the middle alcove, was about 9 minutes, during which the water rose and fell the perpendicular height of about 2 feet. At the western end of the esplanade, however, at 10.30 A.M., and about an hour and a half after high water, the bathing-machines, which had been left dry many feet from the sea, were suddenly floated, and nearly washed away, showing a rise of between 4 and 5 feet.

At Marazion and St Michael's Mount the agitation was observed most of the morning and afternoon.

At Portleven, 7 miles east of Marazion, between 10 and 11 A.M., the sea rushed into the inner basin to the height of about 4 feet, and then rushed out, occupying in this double movement 10 or 12 minutes. The like phenomenon was noticed in the outer basin for some hours, both before and afterwards.

In Falmouth harbour the disturbance must have commenced before daylight, for at 5.50 (local time) it was observed eight miles from its mouth at Truro quay, where, to the great astonishment of the beholders, says the *West Briton*, "a rush of water was seen rapidly ascending from Malpas, three feet in height, which advanced until it reached the head of the river. It was not low water at the time, but there was no tide on. The 'bore' having reached the head of the river, dispersed almost as quickly as it had come. About a quarter to 10 a second rush occurred, but only 2 feet high; and the water, as in the previous case, immediately subsided." Later in the day, when the tide was about two-thirds ebb, a barge left Malpas for Truro, 2 miles distant. On reaching halfway, it grounded and remained stationary about 20 minutes, when it was again floated by an unusually rapid current, which, in a few minutes, rose about 5 feet perpendicularly, and carried the barge up to the railway quay. There the current ceased, and the water immediately receded as fast as it had advanced, leaving the boat again aground. A second influx and efflux of equal rapidity succeeded, but the third and fourth influxes did not rise so high as the first two. The intervals between the successive influxes were about 20 minutes. This information from the bargemen was kindly obtained for me by the master of the steamer "Fal," which plies between Truro and Falmouth. At 5.15 in the afternoon, another rush of the water up the river caught a heavily laden barge at Malpas, and carried it on to Higher Newham, a distance of about a mile. The last that was observed occurred at 6 o'clock, but rose only one foot, barely reaching Boscawen bridge on one side and Truro bowling-green on the other.

At Penryn, another creek in Falmouth harbour, the sea was observed about 8.30 in the morning rushing forward and backward to the height and depth of between 2 and 3 feet, six or eight times in succession, carrying the boats to and fro with great impetuosity. How early in the morning the disturbance was observed at Falmouth I am not aware. At 6 P.M., however, a small steamer then aground, waiting at the jetty head in Falmouth for the ordinary flow of the tide, floated at a most unusually early period; but within half-an-hour afterwards she was again aground, and soon afterwards again afloat. The gentleman who witnessed this then went on board as a passenger to Truro; but about a mile from that town she was unexpectedly left nearly dry in the river.

At Par and Fowey, unusual agitations of the sea were observed on this occasion.

At Looe, the agitation, as the harbour-master writes to me, was noticed from 8 to 10 A.M., the latter being the time of high water. As the sea then rose 2½ feet more than usual, a vessel which had been beneaped floated, and was enabled to leave the harbour.

At Plymouth, in Catwater, this forenoon, the extraordinary agitation of the sea was very great.

In Bridgewater this morning, as stated in the newspapers, "the tide ebbed and flowed three times within a short space. One or two vessels tried to get down the river, but the ebb was so quick that they soon got aground."

At Swansea, "about 10 A.M. (London time), it being then near high water (10.50 A.M.), a reflux of about 1 foot 9 inches occurred, after which the tide again flowed regularly about 2 feet 3 inches—there was no appearance of bore. As our beach dries a mile outside our piers, it is not likely that any disturbance after half tide would be observed." This is an extract from the harbour-master's letter.

In Barnstaple Bay, at Appledore, 2 miles inland, one of the "pilots observed the tide return seven times in succession, the first wave being 2 feet high, the others gradually diminishing." At Bideford bridge (5 miles inland), the harbour-master informs me it was high water at noon, and after it had ebbed or receded 16 or 18 inches, it rose again to high-water mark, and thus ebbed and flowed several times in the space of an hour, accompanied with a strange current. Three miles above the bridge, where the river is much contracted, the phenomenon assumed the form of a wave or bore.

At Bude, " about 9 o'clock in the forenoon, about an hour before high water (says Mr Davey), I was standing on the pier-head, when the water rushed up the harbour until there were 7½ feet of water on the sill of the Lock Gates, 2½ feet above high-water mark; it remained for about a minute, and then rushed back, until there was not water out of the harbour for a boat."

At Padstow, Wadebridge, and Little Petherick, all within Padstow harbour, the agitation was very generally remarked. At Padstow the sea rose from 3 to 4 feet, and the receding and flowing of the water upon the flood-tide were 8 or 10 times within a short period. At Wadebridge, 6 miles inland from Padstow, there were (as stated in the newspapers) 5 or 6 extraordinary influxes this morning, and barges went down the river from that town after the usual tide had receded nearly midway between it and Padstow.

In St Ives Bay, at Messrs Harvey's quay, within the creek of Hayle, a raft of timber was carried up and down a long way several times this morning by the extraordinary alternating current.

In reply to my letters to Kinsale and Havre across our channels, I am informed that no unusual disturbance of the sea had been observed at these places.

About two or three o'clock this morning, when the agitation was first observed at Newlyn, a thunder-storm, with very fierce lightning, visited Mountsbay. The thunder and fierce lightning were observed at Scilly, before midnight, coming from the south, and about 5 in the morning fierce lightning was seen there towards the north-east. This thunder-storm was not felt near Lundy Island, in the Bristol Channel, until 6 o'clock that morning.

The barometer (the daily mean) at the Kew Observatory of the British Association, on the 3d of October, was at a maximum of 30·232, to which it had been continuously rising for the previous six days, that maximum being higher than for twenty-one days before and ever since: on the 4th it was at a minimum of 29·867, lower than for three days before and two days after. The thermometer on the 4th was at a maximum of 76·3, higher than for forty days before and ever since. The sun the day before was most unusually scorching in Mount's Bay, and gossamer webs were very abundant in the air.

It is worthy of remark that the 26th of June and the 4th of October 1859, when the disturbances now described occurred, are more distinguished for their high temperature than any other day of the present year. Remarkably high temperatures also distinguished the agitations here on 5th July 1843, 5th July and 1st August 1846, and 23d May 1847.

We have not had such long-continued oscillations of the sea on our coasts since those of Whitsunday, the 23d of May 1847, and the day following, contemporaneous with which were much more fearful ones at Callao in South America, and an earthquake felt at sea.* This connection between earthquakes and extraordinary agitations of the sea reminds me of a most remarkable fact which has occurred in Mount's Bay since the last meeting of this Society, and which I have noticed in a paper read in May last before the Royal Institution of Cornwall, and printed in the July number of the "Edinburgh New Philosophical Journal," in the library of this Society. "Four hours and five minutes after the *first great earthquake* at Lisbon (Nov. 1, 1755) an extraordinary agitation of the sea commenced at St Michael's Mount. Four hours and forty minutes after the *second great earthquake* at Lisbon (March 31, 1761) another such agitation commenced at the Mount. Four hours and fifty minutes after the *third great earthquake* at Lisbon (Nov. 11, 1858†) a shock was felt about a mile from the Mount, on Tolvaddon Mine. The time on each of these occasions was thus four hours and a fraction after the great shock at Lisbon. The place which suffered most from the earthquakes of 1761 and 1858 is St Ubes, twenty-two miles south-east of Lisbon, and severe shocks were felt at sea many leagues off Capes St Vincent and Finisterre in 1755, 1761, and 1858, indicating that the centre of disturbance was on each occasion beneath the ocean some distance westward of the coast of Portugal, from about which direction the sound heard at Tolvaddon proceeded. It is also very worthy of remark, that as the earthquake at Lisbon on the 11th of November 1858, which lasted half a minute, was divided into two distinct shocks or tremors, so the shock felt at Tolvaddon consisted of two distinct tremors, with an interval of a few seconds between them." The facts mentioned in this paragraph are a beautiful illustration of what was stated by Michell a century since—viz., that earthquakes "generally come to one and the same place from the same point of the compass . . . that the velocity with which they proceed is the same," in the same countries.*

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* See Transactions of this Society for 1850.

† Although my informants were uncertain whether the shock at Tolvaddon was on the 10th or 11th of November, there is little doubt but it must have been the 11th, on which day, at 7.15 A.M., every house in Lisbon was shaken, the earthquake there being greater than any since 1755 (*Times* of 23d Nov.)

An earthquake shock, much more alarming than that at Tolvaddon, passed through the greatest part of Cornwall on Friday the 21st of October inst. (1859), at about a quarter to seven, P.M., visiting, amongst other places, St Austell, Wadebridge, Newquay, St Agnes, Truro, Falmouth, Givennap, Redruth, Ludgvan, and Penzance. It lasted at each place six or eight seconds, and its course appears to have been from S.S.W. to N.N.E., as stated in the newspapers. It was accompanied with a noise like that of "a large engine-boiler passing close at hand over a paved road." At Falmouth and St Agnes it was also attended with fierce lightning of an unusual character, but without thunder. At St Austell many thought their houses were falling, and miners under ground imagined the ground was falling in on them. It is remarkable that on this day, before the shock occurred, the weather in Cornwall and throughout Britain underwent a very great and unusually sudden change; piercingly cold winds from the north and north-west set in, and in the afternoon and evening were several severe hail-storms in Cornwall. At Perranporth, on the north coast, where the cold was extreme, the hailstones in one of these storms were of extraordinary size. Mr Lowe, from Highfield House Observatory, near Nottingham, records that the 21st was a fine and cloudless day—

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* Phil. Trans., vol. lv. p. 566.

intense frost—bar. very low*—faint red aurora borealis at 6½ P.M.—lightning at 7 P.M.—and the greatest cold during the following night was 23·5—intense frost, killing all half-hardy plants. On the 22d there was a violent hail-storm in the afternoon, and the greatest cold the following night was 22·4, with the severest frost ever recorded in October at the Observatory. This temperature was lower than had been ever observed in October; and it is the more remarkable, as the temperature of the 4th of October, when the disturbances of the sea occurred, was *greater* than had been known in the month of October. In the north of Scotland, also, the weather had been comparatively fine until the 21st, the day of the shock, but on the morning of that day snow began to fall very heavily, and by night it lay in many places six or eight inches deep.†

I have thus referred to the two earthshocks in Cornwall since the last meeting of the Society, because I consider that the agitations of the sea now described proceeded from local submarine earthshocks. In accounting for them by such agency, let us take, for example, those at Par on the 25th of June last. Assuming that a considerable portion of the inclined bed of the sea, outside Par harbour, experienced a vertical shock (that is, a rapid succession of vibrations lasting many seconds, as is usual in earthquakes), a large body of water resting on that bed would, by the successive vibrations, be driven seaward, on the same principle that if a smart blow be given to the lower end of an inclined tube filled with

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* The barometer at the Kew Observatory this day was at a minimum of 29.329, lower than for 101 days before and 5 days after.

† The following four coincidences are well deserving of attention :—

Between the agitation of the sea on 25th June last, and this earthquake of 21st October, were	118 days.
Between the two extraordinary agitations in Mount's Bay on 5th July and 30th October 1843, were	117 days.
Between the two agitations in Mount's Bay of 31st March 1761 (the day of the second great earthquake of Lisbon) and 28th July 1761, were	119 days.
Between the two agitations of the sea at Ilfracombe, of 1st November 1755 (the day of the first great earthquake of Lisbon) and 27th February 1756, were	118 days.

118 *days* are almost exactly *four lunations.*

marbles, all the marbles would receive the blow, but only one or two at the higher end would fly off, the rest merely transmitting the blow and remaining stationary. A second blow would in like manner drive off one or two more of the marbles, and so on until the tube was almost emptied. So a blow or single vibration from the submarine ground would pass through the water four times faster than sound through air, and the surface only of the water would be dashed off,* the rest merely transmitting the blow and remaining stationary. A second blow or vibration would drive off a fresh surface of the water, and if the vibrations be repeated ten times every second, for ten or twenty seconds, the fresh surfaces thus driven off in such rapid succession would form a broad and extensive accumulation of water flowing seaward; to replace which the water would retire from the margin of the sea, and thus occasion the efflux with which this phenomenon generally begins. As soon as the momentum of this efflux is exhausted the reaction commences, and the water that had been flowing seaward now flows back to recover its level. This flowing back most probably occasioned the sudden rise in Par harbour, the subsequent ebbings and flowings being merely like the oscillations of a pendulum, which continue until the motion originally imparted is exhausted. The long continuance of the recent agitations, with an occasional increase of intensity, was probably owing to a repetition of submarine shocks.

It is commonly supposed that these agitations on our coasts are the mere effects of corresponding agitations many miles or leagues off at sea. But although there may be, and doubtless generally are, at the same time similar disturbances far off at sea, yet all of them, whether far off or on our shores, are, I believe, perfectly independent of one another, and not one of them extends more than a few furlongs from that particular part of the bed of the sea over or near which the disturbed waters had rested previous to their agitation. An exception, however, must be made in the case of tidal rivers, or where the sea flows far inland. But even in these cases the waters that flowed lately so many miles up the creeks of Falmouth, Fowey, and Padstow harbours do not appear to have been set in motion by any currents outside these harbours, for no current was observed flowing in or out of their mouths, and this agrees with the explanation which I have suggested; for assuming that the submarine sides forming the mouth of either of these harbours incline towards each other, the waters dashed from those sides by the shock would form waves parallel to those sides, and these waves would return to the respective shores from which they had proceeded, so that nothing like a current into the harbour from the open sea would be produced. Inside these harbours, near their mouths, there is at most times of the tide sufficient water, in case of an earthquake there, to fill for a few minutes all the creeks into which the ordinary tides flow.

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* Vessels have often by submarine shocks been arrested in their progress as if they had struck on rocks, and men and even anchors have been dashed up from their decks. On one occasion, 40 leagues west of St Vincent, the men were thrown "a foot and a half perpendicularly up from the deck."—Lyell's Geology, vol. 2, p. 241, 3d edition.

It may appear to some highly improbable that submarine shocks should thus occur so extensively on our Southern and Northern coasts, and yet no shock be felt on dry land. This fact, however, is rather a confirmation of my hypothesis than otherwise; for Humboldt observes that in Chili, Peru, and Terra Firma, the shocks of earthquakes follow the course of the shore—the lowest part of the land—and extend but little inland—and "in the mines of Saxony we have seen workmen hasten up affrighted by oscillations not felt on the surface."* So in England, on the day of the great earthquake of 1755, whilst only one shock was perceived on the surface of the mines in Derbyshire Peak, five were felt there underground, between 11 and 11.20 A.M.,† and on the same occasion ponds were violently agitated without any perceptible shock in their neighbourhoods. Moreover, ducks and geese in ponds have been often observed to rush suddenly from the waters immediately before an earthquake, and Mr Mallet asks whether the reason for this may not be, "that with their heads immersed they are able to hear the first distant mutterings, while yet inaudible through the air."‡ But I imagine that in these cases the bottom of the pond had received a shock which, passing through the water two or three times swifter than a cannon ball through the air, severely struck the birds on the surface, although no shock may have been felt on the adjoining land.

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* Personal Narrative ii., 222, 224.

† Phil. Trans. vol. xlix., p. 398.

‡ Brit. Association Report for 1850, p. 68.

Authors on both sides of the Atlantic, finding that these disturbances of the sea are generally accompanied by thunder-storms or minima of the barometer, have ascribed them to atmospherical causes; but the facts stated in this and my former papers are conclusive against such an idea. Indeed, it appears utterly impossible rationally to account for them by any other agency than that of submarine earthquakes, whether synchronous with known earthquakes on dry land or not. Classing them, therefore, amongst earthquake phenomena, a most interesting inquiry presents itself, viz., why should known earthquakes (as scientific men say) occur equally in all states of the atmosphere, whilst submarine earthquakes not felt on dry land are almost always attended by thunder-storms or minima of the barometer.

Future observers would do service to science by ascertaining—

1st, Whether these disturbances commence with an efflux or an influx.

2d, Whether, when they have in one locality begun with an efflux, they ever in the same locality begin with an influx—and *vice versa.*

3d, At what places they have, and at what places they have not, occurred on a particular occasion, and whether on different occasions the places of their occurrence or non-occurrence are the same.

4th, Whether the submarine ground over and near which they occur be level or inclined, and if inclined, in what directions and at what angles.

5th, The directions in which the currents move.

6th, The distances to which the currents extend seaward from high or low-water line—and the state of the sea outside the disturbed part.

7th, The rapidity, depth, and breadth of the currents, the times occupied by each advance, and by each retreat, and the time between the commencements of two successive influxes or effluxes.

8th, The times of their commencement, greatest violence, and termination at each particular place.

9th, Whether in the mouths of harbours like those of Falmouth or Fowey there is any current flowing out or in from the open sea, or from side to side of the mouth during the agitation within.

10th, The state of the atmosphere, whether calm, or stormy, or lightning, and the states of the barometer, thermometer, and other meteorological instruments, comparatively with their states some days before and afterwards.

By such observations much light would be thrown on this highly interesting branch of geology, and no persons have better opportunity of making, registering, and reporting them than our harbour-masters and coast-guards.

Notes of the Dissection of a Female Beaver. By JOHN CLELAND, M.D., *Demonstrator of Anatomy in the University of Edinburgh.** (With a Plate.)

EXPLANATION OF ILLUSTRATIONS.

Fig. 1. Exhibits the conjoined parotid glands in situ. Above them are seen the masseters and the anterior bellies of the digastric muscles, and the ducts of Stenson passing upwards to enter the mouth in front of the masseters. The sternal attachments of the sterno-mastoids appear below the parotid, and a vein from the parotid on each side is seen crossing the clavicle and dipping between the pectoralis major and deltoid. The pectoralis minor is continuous with the rectus, and on the left side the panniculus carnosus is raised, to show its relation to the external oblique.

Fig. 2. The parotid glands are reflected upwards, so as to show imbedded on their deep aspect *a a* the submaxillary glands, with ducts of Wharton emerging from them, and disappearing between the masseters and anterior bellies of the digastrics; *b* trapezius; *c* sterno-mastoid; *d* sterno-hyoid; *e* omo-hyoid; *f* posterior belly of the digastric; *g* thyroid body. On the left side the sterno-mastoid, sterno-hyoid, and sterno-thyroid are divided.

Fig. 3. Shows the position of the tail and posterior limb when the beaver stands on its hind-quarters. It shows the position of the toes, and the adventitious nail of the second toe; also the manner in which the panniculus carnosus envelops the knee.

Fig. 4. Shows the form of the adventitious nail (left foot).

* Communicated by Dr Douglas Maclagan to the Royal Society of Edinburgh, April 2d, 1860.

Fig. 5. Shows the form of the same structure in a fœtal beaver (left foot).

Fig. 6. Represents the heart and great vessels. It exhibits the long ductus arteriosus, and the dilatation formed by the vena cava and hepatic veins immediately behind the diaphragm. The liver is turned aside to show the size of the vena cava behind the entrance of the hepatic veins.

Fig. 7. Exhibits the castor sacs and genito-urinary organs. The genito-urinary aperture is laid open. *a a* The two halves of the middle lobe of the castor sacs, which has been divided in the middle line; *b* the right castor sac, showing the convoluted appearance of its external aspect; *c* the left castor sac, partially laid open to show the convolutions of its internal lining; *d* orifice of the vagina, immediately above which is the orifice of the urethra, and the clitoris overhanging it. Passing downwards from above the clitoris on each side is seen the abrupt margin of the corrugated lining of the castor sac;—*e* anus; *f* oil-glands of the right side, surrounded by their common investment; *g* oil-glands of the left side dissected separate, and a portion of the largest one laid open to display the orifices of its lobules—three hairs are represented projecting from the common opening of the three glands; *h* urinary bladder; *j* vagina; *k* uterus; *l* fold of peritoneum opposite the ovary, and the Fallopian tube winding in it.

Fig. 8. Shows the muscular tunic of the castor sacs and oil-glands, and the circular space corresponding to the middle lobe of the castor sacs, which is uncovered by the muscular tunic: above it is the pubis, and behind are the muscles of the tail. *a* Round ligament of uterus; *b* transversus perinæi; *c* clitoris.

Fig. 9. Shows the compressors of the rectum, and on the right side the muscular tunic of the castor sac. *a* Transversus perinæi; *b* cut margin of the muscle which passes up to the middle line of the sacrum.

The beaver is an animal so remarkable in form and habits, and has been so long sought after for its fur, and for the once prized castoreum, that it is no wonder that it has attracted much attention from naturalists. But though much has been written, there are still some points in the anatomy of this interesting animal to which additional attention may advantageously be called. For the opportunity of making the following notes, I am indebted to the kindness of Dr Douglas Maclagan, who placed at my disposal, for dissection, the body of a female beaver from the Hudson's Bay territory.

Let us note, first, the immense size of the parotid glands. Their remarkable appearance has excited the wonder of many observers; and a description of them is to be found in "Meckel's Comparative Anatomy,"* along with some account of the mistakes respecting them fallen into by various anatomists. The parotids of opposite sides form one large mass, occupying the whole front and sides of the neck, from the hyoid bone to the sternum, and from the angle of the jaw back to the shoulder. From about the middle of each half emerges a Stenson's duct, which passes over the masseter, and opens in front of that muscle. The glands of opposite sides are quite inseparable. On the deep surface of this mass, and quite embedded in it, are the submaxillary glands, oblong and defined, like large almonds, and with ducts of Wharton about two inches in length (figs. 1 and 2).

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* Meckel, Vergleichende Anatomie, vol. iv. p. 625. (In the French translation the description is inaccurate).

No doubt, the great development of the salivary glands of the beaver is connected with the nature of its food; for it feeds on twigs of trees, and with portions of these cut very small the stomach of the specimen dissected was well filled. In order to reach up to those branches which are above its own level, we are told that it supports itself, kangaroo-fashion, on its hind legs and tail, a posture for which, as we shall see, it is particularly well adapted. In reference to its habits in this and other respects, I may be allowed to quote from a paper in the Philosophical Transactions for 1735,* by Dr Mortimer, in which he gives an account of a female beaver kept for three months in the garden of Sir Hans Sloane. He says, "she was fed on bread and water. Some willow boughs were given her, of which she ate but little; but when she was loose in the garden, she seemed to like the vines much, having gnawn several of them as high as she could reach quite down to the roots. When she ate, she always sat on her hind legs, and held the bread in her paws like a squirrel. In swimming, she held her fore feet close up under her throat, and the claws closed, as when one brings the ends of one's thumb and of all the fingers close together, never moving her fore feet till she came to the sides and endeavoured to get out. She swam with her hind feet only, which had five toes, and were webbed like those of a goose. The tail, which was scaly, and in form of an oar, served as a rudder, with which she steered herself, especially when she swam under water, which she would do for two or three minutes, and then come up to vent, sometimes raising her nostrils only above water."

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* Philosophical Transactions, vol. xxxviii. p. 172. Tab. 430, fig. 2.

The conformation of the beaver agrees exactly with this account of its habits. Its anterior extremities are small, and the distance between the shoulders is narrow, while its posterior extremities are largely developed. The foot and the leg are long, the femur is short, and the muscles of the thigh are of great thickness. The parietes of the abdomen overhang the thigh, and the margin of the panniculus carnosus passes over the knee, so as to include the thigh in the muscular investment that envelops the trunk; and this increases the apparent disproportion between the anterior and posterior bulk of the animal. In swimming, it will pass through the water like a wedge (fig. 3).

When the specimen which I dissected was placed in an upright posture on its hind-legs and tail, there was observed what may be considered as some explanation of the object of the structure which has been described as the additional nail on the second toe of the hind-foot. This so called nail is, indeed, no true nail, but only a horny development of a fold of skin, situated immediately beneath the claw, and above the tip of the toe and the web in which the tip lies. The foot is adapted to the animal's aquatic habits; and not only are the toes webbed, but they are curved inwards, especially the inner toes, probably that they may offer as little resistance as possible to the water while the limb is recovered after the propelling stroke, and also, as it strikes me, that the flexor muscles may be of service in keeping the web of the foot on the stretch, and so assist the interossei. Hence it happens that, when the animal is standing on the ground, especially when it supports itself on its hind-legs, though the three outer toes, which bear the greatest part of the weight, have their plantar aspects sufficiently directed downwards, the two inner toes lie on their sides. The innermost is smallest, and bears none of the weight; but the second toe bears a considerable amount, and, were it not that the horny development protects it, the side of the toe, that is to say, the tender matrix, would be constantly pressed against the ground. But this adventitious growth is so shaped, that while it presents its under-surface downwards, the superior aspect fits in to the concavity of the claw above (figs. 3 and 4).

I have had the opportunity of examining the adventitious nail of a fœtal beaver, about seven inches long, in the possession of Professor Goodsir. It is of an oval form, compressed laterally, presenting a vertical edge, and two symmetrical convex sides, and already the cuticle on its surface is firmer than elsewhere (fig. 5). It would appear, therefore, that the form which this structure has in the adult—viz. flat on the under surface, and turned up at the tip—is not the original shape, but is assumed probably when it begins to be brought into action in walking and standing.

Dr Knox* has directed attention to a peculiarity of the beaver connected with its aquatic habits—viz. a dilatation of the vena cava posterior, like that found in the seal, in which the retarded blood is stored when the animal dives. Its anterior wall is formed by the diaphragm, and the hepatic veins and vena cava behind the liver are also enlarged. Also, as Dr Knox has observed, the obliterated ductus arteriosus is very long and defined (fig. 6).

The castor-sacs of the female beaver appear to have been never as yet sufficiently described. A good deal has been written about the castor-sacs of the male beaver, and Brandt and Ratzeburg† give copious engravings of them; but as to those of the female, there are only a few remarks in the paper by Dr Mortimer already quoted, accompanied by a very inadequate drawing.

If one may judge from comparison of a single specimen with Brandt and Ratzeburg's illustrations, the castor-sacs of the female are decidedly smaller than those of the male. The rectum, vagina, and preputial glands form a tumor about three inches broad and two long, embedded in fat behind the pelvis. The rectum and genito-urinary aperture open into a common depression as in the male, and the arrangement of the oil-glands is also the same as in the male; to wit, on each side there opens into the depression, a little below the anus, an outlet into which fall the ducts of three distinct glands, the largest one most posterior, and the smallest one between the others. These three glands are closely pressed together and surrounded by a dense capsule of connective tissue, as well as by a muscular investment prolonged from the muscular covering of the castor-sacs. They have a light colour and a lobulated appearance. The lobules can be isolated by dissection, and are then seen to surround a membranous sac, into which each lobule pours its contents by a single wide opening, which receives the smaller openings of a number of sub-lobules. A few hairs grow in the orifices of the oil-sacs; a circumstance which agrees with the view established by Liebig, that the lobulated oil-glands found in anal and preputial sacs are an enlarged form of the sebaceous glands of the skin; and doubtless it has the physiological importance of preventing occlusion of the apertures by thickened secretion.

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* Memoirs of Wernerian Society, vol. iv. p. 548.

† Brandt and Ratzeburg, Medizinische Zoologie, pp. 19 and 135, Tab. iv. and iv. *a*.

The general form of the castor-sacs themselves is the same as in the male. They have an outer fibrous wall thrown into shallow winding sulii and convolutions; and within, a firm secreting membrane arranged in much deeper convolutions, and complicated plications, and which, when examined with the microscope, presents a number of small flat tubercles and plate-like elevations, putting one in mind of the minutely cracked appearance of an old and dry painted surface. But it will be seen from the various engravings of male castor-sacs, that the necks of the sacs of opposite sides are united above the preputial opening, *i. e.*, between the prepuce and rectum. Not so the arrangement in the female; the castor-sacs of opposite sides are united by a dilatation which, seen from the outside, is defined by the direction of folds of the outer wall, and looks like a small third lobe, and which lies on the inferior or pubic aspect of the genito-urinary aperture. The clitoris makes its appearance on the anterior margin of this dilatation, immediately inferior to the urethral orifice. The urethra is very long, about three inches in the specimen dissected, and the vagina has about the same length (fig. 7).

The castor-sacs are covered with a muscular investment, somewhat differently arranged from that in the male. The fibres are strongest on the superior or rectal aspect, where they arise in common with the compressors of the rectum. Those most posterior pass transversely outwards over the extremities of the castor-sacs and oil-sacs, and some of them between the two, then turning round are inserted inferiorly into a set of circular fibres that surround an uninvested space in the middle line, which corresponds to the median dilatation. The fibres arising further forwards slope backwards more and more to reach the inferior aspects of the sacs external to the circular fibres; and the foremost fibres arise from the pubis, and pass directly backwards. From the inner aspect of the ischium, on each side, two muscles arise, one outside the other, and pass backwards and inwards to be inserted into the longitudinal fibres of the rectum and the muscles of the castor-sacs. On each side a long slender fibrous band passes back from the abdominal wall to be inserted into the anterior part of the muscular tissue of the castor-sacs; it is the round ligament of the uterus (fig. 8).

The compressors of the rectum are so remarkably developed that I have made a drawing of them. Posteriorly lies the sphincter ani. Further forwards, on the superior aspect of the rectum, is a superficial muscle arising in the middle line, and passing backwards and outwards. Partially covered by the above, a powerful muscle arises also from the middle line of the superior aspect, which it occupies for nearly two inches, and its fibres, passing downwards and forwards, embrace the rectum, and are inserted in close connection with the tunic of the castor-sacs. In front of this muscle is a ring of circular fibres; and still further forwards the rectum is embraced by a muscle which passes downwards and backwards from the middle line of the sacrum (fig. 9).

Notice of a recent Longitude Method for Travellers, proposed by Colonel EVEREST, B.A., *late Superintendent of the Great Indian Trigonometrical Survey.*

While the subject of longitude for sailors has been so abundantly discussed and brought out in nearly every possible form, there has not been quite so much attention paid to the methods most proper for travellers on land. Hence many a wanderer in the interior of some barbarous continent has simply provided himself, in beginning his journey, with the same instrumental means which are employed by a navigator at sea; and has found, after much practical toil and disappointment, that they are not the most convenient or effective forms of apparatus for a person in his terrestrial circumstances; even with the assistance of that usually very troublesome apparatus, an artificial, or, as some delight to call it, a false, horizon.

Moreover, when a sextant, the sea instrument *par excellence*, is held in the hand on shore, as the traveller imitating the sailor tries to do, a considerable amount of art is necessary to use it effectually, and it is well known—" 'Tis true 'tis pity, and pity 'tis, 'tis true,"—that some men never learn the proper knack; although, too, they may be good, stout, able-bodied fellows, with a fair mathematical education; and no want of general practical ability either, whenever an instrument is of plain speaking character, looks straight at an object instead of round a corner, by aid of prism or reflector; and, above all, when it is mounted securely on a tripod stand.

Now these are the conditions of a theodolite, the type of the landsman, as the sextant is of the sailor; and to help those who can work the former self-supporting class of instrument, and that only in a respectable manner—Colonel Everest, who in the course of his magnificent Indian Surveys has used theodolites as well as other astronomical and geodetic apparatus for forty years—is now contributing data to the Royal Geographical Society, which will make the theodolite, verging towards the alt. azimuth instrument, as easy, if not to many much easier, than the sextant on shore.

The colonel very properly looks forward, in all long travels through unknown countries, to chronometers failing, and astronomical methods being alone capable of yielding trustworthy longitudes; while, amongst such methods, he gives the highest place, beyond all compare, to "lunar distances;" as probably do almost all other men of much experience. The only room therefore for doubt is in the question, How is this quantity, the lunar distance, so immediately obtainable by a sextant, to be procured with a theodolite?

To this end, Colonel Everest proposes to *measure* the differences of alt. and azimuth of moon and star, and from those data, *compute* the distance. In order to promote accuracy, as well as facilitate the method, he gives examples of the order in which the observations should be made, first a star, then the moon, and then another star on the other side; after which the instrument is to be reversed 180°, and the measures repeated with the face of the vertical circle in the opposite direction; and he suggests that a large stock of printed forms might be kept, in order that every intending traveller might be fully supplied with those excellent means for promoting precision and accuracy in recording observations.

He then enters into an able disquisition on the methods of computation to be adopted, to assimilate the results with those of the usual lunar distances of the seamen, so as to enable the further reductions to be performed with the same tables; and concludes with hoping that those who have hitherto not fixed their routes in longitude, from disliking sextant methods, may take more kindly to the theodolite used in this manner, and find their advantage therein.

Certainly, should British sappers and miners be again taken from their theodolite work on the Ordnance Survey, and sent to accompany learned men into the interior of Africa, as was the case with Dr Vogel's party, they might be expected to get into the practice of Colonel Everest's alt. azimuth lunar distances much sooner than into the nautical reflecting method.

C. P. S.

On Nerve-Force. By H. F. Baxter, Esq.

Is nerve-force a polar force? If so, in what respects does it differ from other polar forces? or what evidence have we that it is a polar force at all? These are questions which no doubt arise in the minds of many physiologists of the present day. Without entering into any detailed account of the various opinions that are entertained in regard to the nature of nerve-force, it may be fairly considered that those who entertain the opinion that nerve-force is a polar force are called upon, in the present state of the question, to give some direct experimental evidence in support of their conclusion: the *onus probandi* evidently rests upon their shoulders. We cannot remain satisfied with the vague notions that animal life and electricity are identical, or that nerve-force and electric force are identical, or that nerve-force differs entirely from electric force, or that vital forces are totally distinct from inorganic forces. The mind will not and cannot rest satisfied with these vague assertions: it requires something more definite. We want to know, if possible, how far they agree, and in what respects they differ. It will be seen that, to answer these questions satisfactorily, some of the most difficult physiological problems are now presented for our consideration; and although we might not be enabled to succeed in solving them to the extent we might wish, it is to be hoped nevertheless that the attempt will be neither useless nor unprofitable.

The first question that will arise is the following:—*Can we detect any manifestation of* CURRENT FORCE *in nerves* DURING *nerve-action?*

It will be necessary to make a few preliminary observations respecting the employment of the terms *nerve-action* and *nerve-current.*

The fact that current force exists in a nerve is well known, and may be shown, as was first pointed out by Du Bois-Reymond, by placing the electrodes of a galvanometer, one in contact with the *transverse* section of the nerve, and the other in contact with the *longitudinal* section or side of the nerve: this has been designated as the *nerve-current.* So far this nerve-current does not differ from that which has been called the *muscular current,* and which may be obtained in the muscle in the same manner. I have been led to consider, in a previous paper,* that this so-called *nerve-current* is dependent upon *nutrition.* It does not afford direct evidence that the force which exists in the nerve as nerve-force, and which may be supposed to be transmitted from one part of the system to another along the nerve, is *current force:* it only proves that the nerve-tissue, like the muscular tissue, is in an electric condition—a condition no doubt necessary and essential for nerve-action as well as for muscular action. Current force is not manifested when the electrodes are placed at the extremities of a nerve, the nerve being in a quiescent state; and this fact does not prove that the force transmitted by the nerve *during* nerve-action is *not* current-force. The real question therefore is, whether *during* nerve-action, during the passage of nerve-force along a nerve, current force is manifested or not. We must take care, however, and bear in mind that the term *nerve-current* may be employed to express two distinct ideas—1*st*, The *electric current* manifested in the nerve, and which indicates the electric condition of the tissue; and, 2*d*, The *force—nerve-force*—which is transmitted along a nerve *during* nerve-action.

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* Edinburgh New Philosophical Journal. New Series. Jan. 1858.

Vavasseur * and Berandi,* and David,* appear to have obtained some results in their experiments when the electrodes of a galvanometer were inserted into different parts of a nerve. These experiments, however, were undertaken prior to the knowledge of the existence of the nerve-current, and the effects then observed may have been due to the electrodes having been placed on different parts of a nerve, the result being the so-called nerve-current of Du Bois Reymond.

The experiments of Pacinotti† and Puccinotti,† repeated and confirmed by Matteucci,† must not be passed over. These inquirers inserted one electrode into the brain, and the other into the muscles, when a deviation of the needle to a large amount occurred, the electrode in contact with the muscle being *positive* to the other. In this instance, it might be supposed that the current travelled *from* the brain along the nerve *to* the muscle. I have alluded to these experiments in a former paper;‡ and as similar effects upon the needle were obtained when the electrode was inserted into the internal jugular vein instead of the muscle, I have been led to suppose that the effects are due to the electric condition of the nervous tissue, the result of nutrition, and not as indicating the passage of an electric current along the nerve.

Prevost § and Dumas,§ Person,§ Muller,§ and Matteucci,‖

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* Muller's Elements of Physiology; translated by Baly. Second edition, vol. i. p. 685.

† Traité des Phénomènes Électro-Physiologiques des Animaux, par C. Matteucci. 1844. P. 119.

‡ Philosophical Magazine, Jan. 1856. § Muller's Elements, pp. 686–689.

‖ Traité des Phénomènes Électro-Physiologiques, p. 253.

have failed to obtain any evidence of current-force being manifested when the electrodes of the galvanometer were inserted in the nerves of a living animal. Some of my earliest experiments were undertaken for the purpose of ascertaining this point; and although results were occasionally obtained, nevertheless, as the experiments were performed prior to the knowledge of the existence of the electric condition of the nervous tissue, I have no doubt that the effects then observed were due to the circumstance that the electrodes were placed on heterogeneous parts of the nerve, as I shall be able to show presently. On the present occasion, only my more recent experiments will be related.

The experiments may be classed under three heads:—1*st*, Those in which the *galvanometer* was employed; 2*d*, Those in which the *galvanoscopic frog* was used; and, 3*d*, Those in which a *magnetized needle* was used.

Sect. 1.—*The Galvanometer.*

The animal, most frequently a rabbit, but occasionally a guinea-pig or a frog, was pithed or rendered insensible by means of prussic acid. The sciatic nerve was carefully exposed throughout its whole course, and a plate of thin glass passed beneath it. All traces of blood being carefully removed, the pointed extremities of platinum electrodes were inserted at the extreme ends of the exposed nerve, as far apart as possible, leaving, however, a small portion, just at its exit from the pelvis, for the purpose of serving as a point of irritation. The leg of the limb was fastened down so as to prevent a too great motion during contraction, which might otherwise disturb the position of the electrodes. The other ends of the electrodes rested on and dipped into wooden cups containing mercury, the cups being placed upon glass for insulation: by these means a slight motion of the electrodes would not disturb the galvanometer, and it remained perfectly steady. The galvanometer employed has been already described on a former occasion.*

At first the nerve was irritated with the point of a steel needle, which was insulated, being inserted by means of a cork into a glass tube, which formed the handle; but, as may be readily supposed, the action of the steel needle upon the needles of the galvanometer so interfered with the results, that it was obliged to be set aside, and a pointed piece of copper wire, or a piece of glass or a glass pen, was used for the purpose.

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* Philosophical Magazine, Sept. 1855.

The experiment thus arranged, the nerve was then irritated, and the muscles of the limb made to contract; but no effect occurred upon the needle. When, however, one electrode remained in the nerve, and the other was placed on its external surface, then the ordinary effect, the *nerve-current*, was produced, the nerve during this period not being stimulated to action: if the nerve was stimulated and the muscles contracted, there did not appear to be any effect upon the needle indicative of an *increase* in the *nerve-current*, or even of a *sudden decrease* in it, the needle gradually receding to its former position. The nerve was divided at its lower extremity, one electrode brought into contact with the divided surface, and the other with the longitudinal surface; the effects were the same; the *nerve-current* appeared, but after that there was no *increase* or indication of a current upon stimulating the nerve.

It will be readily seen that two questions are involved in this last experiment, 1*st*, Is a nerve, during nerve-action, traversed by a current of electricity? which we are now considering; and, 2*dly*, Is the *nerve-current* affected during nerve-action? a question which will be considered further on.

In other experiments the abdomen was laid open, so as to expose the lumbar plexus of nerves, and the nerves excited by a current from a pair of Grove's cells, so as to produce a more powerful contraction of the muscles, the effects were negative, so long as the electrodes remained in their first position *in* the nerve; but if, from the motion of the limb, or intentionally, the electrodes were moved, so as to be in contact with heterogeneous parts of the nerve, then vibrations of the needle were occasionally produced. The spinal cord was irritated in the lower part of the dorsal region, by passing a copper wire between the vertebræ, so as to excite contraction, but the results were still the same. The nerve was excited by touching it with caustic potash, without any effect being produced upon the needle of the galvanometer; but if the alkali came into contact with one of the electrodes, then an effect occurred upon the needle, evidently due to the chemical action thus set up.

The animal was poisoned with strychnine, and as soon as tetanic contractions occurred, the experiment was repeated, but with the same negative results.

The only conclusion to be drawn from these experiments is the following:—That *when the electrodes of a galvanometer are inserted* IN *a nerve during nerve-action, there is no manifestation of current force; but if the electrodes come in contact with heterogeneous parts of the nerve, during nerve-action or not, then current-force is manifested; this effect being, however, the result of the so-called* NERVE-CURRENT.

SECT. 2.—*The Galvanoscopic Frog.*

The galvanoscopic frog is, unfortunately, accompanied with great uncertainty in its indications, and consequently requires the greatest care in its employment. The delicacy of its indications does not always correspond with its freshness; the muscles may be easily excited to contract when first prepared, but immediately afterwards it may be difficult, or even impossible, to arouse the contractions. This difficulty may arise from the muscles having once contracted remaining so. Again, a limb which is not very delicate in its indications at first, may, after some little time, be all at once seized with slight tetanic contractions; this circumstance may arise from the nerve becoming dry, as has been already pointed out by Marshall Hall.* There are other circumstances which are undoubtedly influential, and amongst them the vital condition of the animal. It has frequently happened to me, that a limb which has been laid aside as worthless, has subsequently proved upon trial to be most delicate in its indications, without my being able to account for it. The muscles should be neither flabby nor too much contracted. The only test to be relied upon is, by occasionally trying the limb with some well-known source of electric action, such as the muscle or nerve, and to have several at hand in case of emergency.

The former experiments were now repeated, and the nerve of the galvanoscopic frog, its limb being supported on a piece of glass, was laid at one time transversely on the sciatic nerve, at another time longitudinally on the nerve, and the nerve then irritated as before, but no effect occurred upon the galvanoscopic limb. If the galvanoscopic nerve be placed carelessly upon the other nerve, so that its end and side touch the nerve, and thus form a circuit, then the galvanoscopic limb will contract, this being the result of the nerve-current in the galvanoscopic frog acting upon its own muscles; the effect being over, and the nerve remaining in this position, no contraction occurred upon stimulating the sciatic nerve, but upon *opening* the circuit of the nerve, it occasionally happened, if the frog was delicate in its indications, that contractions ensued. Should there be any blood upon the nerve of the animal, and the nerve of the galvanoscopic limb touch it and the nerve, then the galvanoscopic limb will contract; this may be considered as the result of a current arising from the contact of heterogeneous substances. But in neither instance was the effect anything like that which occurs when the galvanoscopic nerve is placed upon a tetanized muscle.

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* Edinburgh New Philosophical Journal, April 1848.

Neither Muller* nor Matteucci* have been able to obtain any effect when they placed the galvanoscopic nerve upon another nerve in a living animal.

The sciatic nerve of the animal was now divided at its lower extremity, and the nerve of the galvanoscopic limb arranged thus: its divided extremity was placed in close contact with that of the animal, and then brought round so that its side should be in contact either with the side of the nerve of the rabbit, or with that of its own nerve, forming a loop. Now, if there was anything like a current passing along the excited nerve of the animal, we should expect it to be continued on along the nerve of the galvanoscopic frog, producing the effect of a *direct* current. The galvanoscopic limb contracted when first applied, due either to its own nerve-current, or to that of the animal, but no contraction occurred during the stimulation of the nerve, unless the circuit was broken. Instead of the side of the nerve of the galvanoscopic frog being placed in contact either with its own nerve, or that of the animal, the tendo Achillis was used: the effects however were still the same, no contraction during the stimulation of the nerve. When the end of the nerve of the galvanoscopic frog was merely placed in contact with the end of the sciatic nerve, so as to form a prolongation of the nerve, no contraction of the galvanoscopic limb occurred. The divided ends of the sciatic nerve of the animal were brought into as close contact as possible by bending the limb upon the thigh, and the nerve then stimulated at its upper part, but there was no contraction produced in the lower limb.

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* Loc. cit.

The same conclusion may be drawn, and the same remarks made, in regard to the galvanoscopic frog, as were made in regard to the galvanometer, viz., *when the proper precautions were taken, no evidence indicative of the manifestation of* CURRENT-FORCE *in a nerve* DURING *nerve-action could be obtained.*

SECT. 3.—*The Magnetic Needle.*

It may be readily supposed that, if we failed to obtain any evidence of the existence of current-force in a nerve during nerve-action by means of the galvanometer, it would most probably happen that the magnetic needle would also fail to detect it. To exhaust every possible mode of its detection, the following experiments were undertaken. I may just add, that Prevost,* Dumas,* and Muller,* have already performed similar experiments, but failed in obtaining any result.

A small magnetised needle, three quarters of an inch in length, was suspended by means of a single fibre of silk-worm silk, the needle being passed through a strip of card, and to this the silk was attached. To avoid motion of the needle from slight draughts of air, and from vibration of the room, the silk was attached to a firm support, and a glass tube an inch in diameter, and five inches in length, was so arranged as to inclose the needle. The sciatic nerve being laid bare, a plate of glass was placed beneath it, and thus the nerve was elevated above the surrounding muscles; it was then brought beneath the magnetic needle, and kept in that position, being

* Loc. cit.

supported upon a stool about a quarter of an inch below it. The nerve was placed in various positions with regard to the needle, sometimes parallel, at other times transversely or obliquely to it. The needle being perfectly steady, the muscles of the leg were made to contract, as in the previous experiments; but, in whatever manner the experiment was arranged, there was no indication of any action upon the needle. Whenever any motion of the needle occurred, it was evidently due to the motion of the atmosphere produced by a too great motion of the limb. I may also observe that, as the experiment was performed near a window, it was necessary to guard against the heating effects of the sun's rays upon the air within the tube. The importance of attending to this latter circumstance in these experiments may be seen from the effects observed in a former series of experiments, to which I must refer.*

Can we magnetise a needle? Thick and thin needles about three quarters of an inch long, and free from magnetism, were inserted either *transversely* or *longitudinally* in the nerve, and the muscles then made to contract powerfully and for some time by means of an electric current from two of Grove's cells. In these experiments there was no distinct evidence of the needles having become magnetised. Upon testing the needles after the experiment with steel filings, it was frequently observed that the filings adhered to the needles throughout its whole extent, although they had been previously wiped with a dry clean cloth. These effects were evidently due to the needle being damp; the moisture from the fingers was sufficient to produce this damp state, and the only mode of preventing it was by heating the needle, but by so doing any magnetic state it might possess would be destroyed. The difference observed in the needles in which the filings adhered when magnetised or when damp is very great, in the latter the whole surface is slightly covered with them, but in the former the filings are confined to one or two spots.

The results of our present investigation only tend to confirm the opinion already expressed by Muller, Matteucci, and others, that *nerve-force* is not identical with *current-force.* Or the conclusion may perhaps be more correctly expressed by saying that *we have not been enabled to obtain any evidence of the manifestation of current-force in a nerve during nerve-action.* It must be borne in mind, however, that we are now speaking of nerve-action, and that we are not disproving the existence of the so-called *nerve-current* which is manifested in a nerve during its quiescent state; and this brings before us another most important question for consideration, viz.,—*Is this nerve-current affected during nerve-action?*

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* Edinburgh New Philosophical Journal, July 1856.

In De la Rive's valuable Treatise on Electricity,* and to which I must refer, as containing perhaps the most recent views in regard to this question, will be found the following important remarks:—"We know," says De la Rive, "that the nerve possesses of itself a certain electrical state, which we have succeeded in determining; we know, moreover, that this electric state is modified by every excitation exercised upon the nerve. . . . . Now, if, by any course whatever, the electric state of the nerve is modified, equilibrium is destroyed; and from this there results a contraction of the muscle, or a sensation. Before studying the consequences of the modification, we may remark, that it consists in the fact that the organic molecules of which the nerve is formed are not polarised transversely from within outwards, but longitudinally from one extremity to the other, as is every conducting body traversed by an electric current. When the modification arises from the immediate action of the nervous centre, it appears that the polarisation is brought about always in such a manner that the negative poles of the molecules are turned on the side of this centre, and the positive on the side of the muscle, as would result from the action of an electric current that might be travelling in the direction of the nervous ramifications. This it is that explains why an electric current which travels in this direction favours the contraction much more than when it travels in the contrary direction. This is equally a natural consequence of the fact that the particles of the nerves upon which the immediate action of the brain must be exerted, being the interior which penetrate into it more deeply, have their negative poles free."

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* A Treatise on Electricity, by Aug. De la Rive. Translated by C. V. Walker, F.R.S., vol. iii. p. 56.

"If, instead of coming from the brain, the action exerted upon the nerve comes from the muscle, the polarisation of the nerve must take place in a contrary direction, namely, so that the positive poles are all turned towards the side of the nervous centre, and the negative towards the side of the muscle whence the excitation comes." I have quoted these observations at some length as containing perhaps the most recent views upon the subject and supported by an authority of some weight. Now, the only conclusion that appears to me that can be drawn from these remarks is this, that the force propagated along a nerve during nerve-action is identical with current-force as it exists in a wire carrying a current of electricity. If so, I need scarcely add, that we ought to be enabled to obtain some direct *experimental* evidence in support of this opinion: at the present time I know of none beyond that advanced by Du Bois Reymond, who appears to have ascertained in his experiments that the *nerve-current* may be made to *increase* or *diminish* according as the nerve is excited by an electric current—being increased if the current passes in one direction, and diminished if it passes in the contrary direction.

I have already had occasion to relate some experiments on a former occasion* in which I endeavoured to obtain an *increase* of the nerve-current according to the mode suggested by Du Bois Reymond, but entirely failed in doing so. As those experiments were not undertaken for the express purpose of ascertaining whether the nerve-current is affected during nerve-action, the following experiments were performed.

Is the nerve-current affected during nerve-action? Instead of employing a nerve separated from the animal, the experiments were conducted in a manner similar to those that have been already related in the previous part of the present paper, viz., with the sciatic nerve. The electrodes of platinum were coated at one extremity with shell-lac, leaving, however, the extreme end bare, and one of them was pointed so as to be easily inserted into the substance of the nerve, whilst the other electrode presented a flat surface to rest upon the surface of the nerve. The distance at which the electrodes were placed from each other, varied from an inch to a quarter of an inch, but were generally within about half an inch of each other. When the needle indicated the existence of the nerve-current, the upper end of the sciatic nerve was stimulated either by means of the glass pen, or copper wire, or an electric current, to produce muscular contraction in the leg: sometimes the nerve was stimulated by a constant current, at other times by an intermitting current. The current was passed at one time as a *direct* current, at another time in the *inverse* direction. Now, in whatever manner the nerve was excited to action, I failed to obtain any evidence of a decided *increase* in the *nerve-current,* neither could I obtain any definite indication of a *sudden decrease* in the nerve-current—the needle gradually receded. Vibrations of the needle were frequently observed, and were evidently due to the motion of the electrodes caused by the movement of the limb during the contraction of the muscles. The effects upon the needle were just the same in whatever position the electrodes of the galvanometer were placed; whether the electrode in contact with the surface of the nerve was placed on the upper or lower portion of the nerve, between the stimulated portion of the nerve and the other electrode, or below the latter. There was no decisive action upon the needle in these experiments indicative of any marked influence over the nerve-current, the needle merely returned to its former position, or gradually receded.

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* Edinburgh New Philosophical Journal, April 1858.

When the galvanoscopic frog was employed, as in SECT. 2, after the first effect of the nerve-current was over, there was no further contraction, however long the nerve was stimulated; there was no effect corresponding to the tetanized muscle—it was impossible to produce a tetanic condition of the nerve, so that it should affect the nerve of a galvanoscopic frog.*

I should certainly hesitate before coming to the conclusion that *no* effect is produced upon the nerve-current during nerve-action; but I certainly have not been enabled to obtain those definite and constant indications that we have a right to expect under the supposition that the nerve-current, as manifested in the *transverse* direction of the nerve, is converted *during* nerve-action into a current in the *longitudinal* direction of the nerve, which appears to be the opinion of De la Rive, as expressed in the remarks I have already quoted.

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* What the condition of the nerve may be along which the *inverse* current has passed for some time, so as to produce tetanic contractions in its own limb, I have not been able to ascertain; there is no *increase* in the nerve-current.

Concluding Remarks.

The conclusions that we have arrived at in the present investigation being of a negative character, may perhaps be considered as anything but satisfactory. Let us not, however, be led away by the false supposition, that because negative results have only been obtained, that therefore no positive knowledge is acquired: we may have ascertained a most important fact, if true, and, whether true or not, may partly depend upon our being able to give a satisfactory reason for our failures. Have we not, it may be asked, commenced our inquiry with a false notion in regard to nerve-action—viz., its identity with electric action? Have we not supposed an identity to exist between current force and nerve-force, which we have failed to prove? To suppose that the conditions may exist in the one case and not in the other, and that the two forces may still be the same, cannot be deemed satisfactory to any experimentalist. If nerve-force be electric force we have a right to ask for some proof of it. The circuit form of the arrangement exists in the *transverse* direction of a nerve, and we can detect the necessary current, the so-called nerve-current. The effects here, however, are analogous to those of a charged Leyden jar rather than to a voltaic circle, as I have endeavoured to show in a former paper, and to which I have already referred. The very circumstance of our being able to show this state in the *transverse* direction, would lead us to expect that we ought to be able to prove its existence in the longitudinal direction, if it existed.

The subject under consideration involves, however, three distinct questions. 1*st*, Is nerve-force nothing more than the electric force which exists in the nerve, and put into motion *during* nerve-action? or, 2*dly*, Is this electric force in the nerve *converted* into nerve-force during nerve-action? or, *3dly*, Is this electric condition of the tissue merely a condition, and perhaps a necessary condition, for the manifestation of nerve-action?

In reply to the first question, it may be observed, that my experiments have failed to give the necessary proof in favour of this supposition.

In regard to the second question, if we could obtain any decided evidence of the nerve-current being affected *during* nerve-action—I say decided—then we should be called upon to account for its *increase* or *decrease*, according to the principles of *conservation of force*,* and to show what has become of the force. But if the effects that have been obtained with the galvanometer, viz., a *gradual decrease* in the nerve-current, such as I have observed, be due merely to a disorganisation of the nervous tissue, and they are such as are consonant with this supposition, then nerve-force must be ranked as a higher *form* of force, and the electric condition of the tissue merely a condition for the manifestation of nerve-action. To assist in elucidating this question, let us just refer for a moment to the muscular current, and see whether this current is affected during muscular contraction.

When the electrodes of a galvanometer are so arranged with the muscular fibre, viz., in contact with the transverse and longitudinal sections, the muscular current is produced; upon making the muscle contract the needle returns rapidly to its former position, and passes beyond it. Du Bois Reymond has designated this effect "the negative variation of the muscular current;" the exact meaning of the phrase I do not comprehend, if it be intended to express more than the fact. The question, however, is this, Is the return of the needle in consequence of one of the surfaces of the muscle separating from the electrode during the contraction of the fibre, and so breaking the circuit; or is there a sudden diminution in the electric tension of the tissue, and so producing a *decrease* in the muscular current? Now, we have some evidence that there is a discharge of electric force during muscular contraction, as shown by the galvanoscopic frog in Matteucci's experiments; but we have no evidence that the muscular current is increased in these experiments during muscular contraction. The loss of force—the lowering of the electric tension of the muscular tissue—which takes place during contraction is restored by nutrition.* Now, if we can detect the evolution of electric force in the muscular tissue during muscular action, surely we ought to be able to obtain some evidence of the manifestation of electric action in the nervous tissue during nerve-action—if nerve-action be, as it is supposed to be, merely the result of the electric force of the tissue being converted into current force.

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* On the Conservation of Force. By Prof. Faraday. "Philosophical Magazine," April 1857.

Let us not hastily conclude that nerve-force is totally distinct from electric force, or that it bears no relation to it or the other *polar* forces, such as magnetism, for example. We appear to be in a position somewhat similar to the physical philosophers prior to the discovery of Faraday of magneto-electricity. So long as they confined their experiments to the mere application of the electrodes of the galvanometer to the two ends of the magnet, no result was obtained; the necessary evidence, the connecting link, was wanting; and we may now be in a somewhat similar position. I am not now supposing, or going so far as to say, that it *must* be by means of the galvanometer that this necessary evidence, the connecting link, is to be obtained; it *may* be by some other fact totally unconnected with the galvanometer that the connection will be shown. We have sufficient evidence, however, to show that an intimate *connection* exists between nerve-force and electric force, viz., in the development of the electric force in the fish, in the dependence of the former upon the *will* of the latter. We may also refer to the relation that exists between nerve-force and muscular force. We have strong evidence for believing that muscular force is a *polar* force; the electric condition of the tissue and the development of the force during contraction is in favour of the supposition. We are not compelled to assume that any peculiar force exists, or is associated with the muscle distinct from this electric force, only that the mode in which it exists is brought about by other agencies than those that occur in the inorganic kingdom, viz., by *nutrition*. It is in these intimate relations and connections in the dependence of the development of electric force in the fish, and of muscular contraction upon nerve-action, that the strongest evidence for the *polar* character of nerve-force is manifested, and not so much upon the electric condition of its tissue.*

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* According to the principles of the *Conservation of Force* we must endeavour to trace out in what manner the force in the muscle is disposed of. We have *heat* developed during muscular contraction, as shown by Becquerel, and Breschet, and Matteucci. Carbonic acid is evolved during muscular contraction, as shown by Matteucci in his experiments on muscular respiration. Electric force is evolved during muscular contraction, as has been proved by Matteucci, and confirmed by others, and also by some of my own experiments. It would be, perhaps, erroneous to suppose that the force is converted into chemical action as nutrition; nutrition is undoubtedly increased during continued muscular exertion; but muscular action—contraction—would appear to be the first act in this series of events, and nutrition the second; contraction may be considered as the exhausting act, nutrition the restoring act, in this process.

But, it may be asked, are we justified in supposing that nerve-force, in nerve-action, is entirely independent of the electric force as it exists in the nerve in its quiescent state? Do we not see that nerve-force bears some relation to the state of the vital powers of the animal, viz., its nutrition; as nutrition is increased or diminished, does not nervous energy increase and diminish in a corresponding ratio, and have we not *experimental* evidence to show that the electric state of the tissue (the nerve-current) is dependent upon, and varies according to, the state of nutrition of the nerve-tissue. Do not all these facts show that an intimate relation exists between these two forces—between the electric force in the tissue and nerve-force, and therefore what right have we to suppose that any other force but the electric force is connected with the nerve.

All this may be granted, but the premises do not justify the conclusion. There may be no necessary connection between the electric state of the tissue and nerve-force beyond that of being a condition, and perhaps a necessary condition, for the manifestation of nerve-force. I will not go so far as this, and say that the electric force of the nerve has *no* connection whatever with nerve-force, but only this, that nerve-force is *not* merely the electric force of the tissue converted into current force, as has been supposed to be. Whether the electric force of the tissue is *converted* into nerve-force *during* nerve-action is another question, and perhaps the question for us to solve. At present, we have no *experimental* evidence to prove this supposition; for we have not obtained any indication of a decided loss in the electric state of the tissue, a *sudden decrease* in the nerve-current *during* nerve-action to indicate such a *conversion*. This evidence, however, is not decisive of the question, and at present it may be considered an open one.*

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* We must not overlook the connection which exists between nerve-force and *secretion*. The latter is undoubtedly a *polar* action, but the influence of nervous action over secretion, although it undoubtedly exists, does not appear to be exerted in so immediate and *direct* a manner as in the two instances we have just quoted.

It must be remembered that we have hitherto limited our views, and have been comparing nerve-force with only one of the ordinary polar forces with current-force. Let us now compare it in regard to magnetic action. In a magnet we can get no evidence of current-force travelling *from* one pole *to* the other; the force exists in a state of tension, which may be raised or lowered without affecting the galvanometer when arranged with the electrodes at the two ends. May not nerve-force exist in a state of tension, and nerve-action correspond with a rising or lowering of this state, just as the muscular fibre may be regarded as existing in a state of electric tension and muscular contraction, the result of a lowering of its tension. The development of the force in the fish, and other facts, may perhaps be adduced as an argument against this supposition, and in favour of transmission of force *from* one point *to* another along the nerve. Still it behoves us not to keep limiting our views to one class of actions only, but to be bold and suggestive, and seize upon any resemblances, however slight; and provided we do not allow them to take a too firm hold of our minds, so as to lead us to consider them as realities, but merely suggestive, by leading to future experiments, we may hope to arrive ultimately at some more promising and positive results.

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* I have not thought it necessary to adduce other well-known evidence to show the difference between the action of nerve-force and of current-force. The ligature of a nerve preventing the transmission of nerve-force, is a strong argument against the identity of these two forces.

The following conclusions may be deduced from the foregoing inquiry:—

First, That nerve-force, *during* nerve-action, is not *current*-force.

Secondly, That the electric condition of the nerve, as manifested by the nerve-current, is not *converted* during nerve-action into current-force.

Thirdly, That the electric condition of the nerve may be merely a condition, and perhaps a necessary condition, for the manifestation of nerve-action.

Fourthly, That the evidence in favour of nerve-force being *polar*, is shown by the *connection* that exists between the development of the electric force in the fish and its dependence upon the will of the animal, and also in the connection between nerve-action and muscular action, the latter being regarded as *polar*.

Fifthly, That the whole of the evidence indicates that nerve-force is of a higher character than any of the other known *forms* of *polar* force; and,

Sixthly, The question whether the electric force, as it exists in the nerve, may not be *converted* into nerve-force *during* nerve-action, may be considered at present an open question.

Remarks on Ozone. By ARTHUR MITCHELL, A.M., M.D.*

The object of the present short communication is to expose some imperfections in that mode of determining the amount of ozone in the atmosphere which is at present in use among Meteorologists.

In 1848 Schönbein discovered this peculiar condition of oxygen, and showed that it existed in the atmosphere in an uncombined state. He at once concluded that a substance possessing such properties as those belonging to ozone ought to have important bearings on animal and vegetable health, and he was thus led to search for a method of measuring its varying quantity in the atmosphere. This search resulted in what is now known as Schönbein's Ozonometer. Paper is dipped in a solution of iodide of potassium and starch, and then dried. A slip of this is exposed to the action of the air. If ozone be present, it liberates the iodine, which combines with the starch, and gives a blue colour to the paper when it is moistened. This is then compared with a scale, having on it different shades of blue, numbered from 1 to 10, and the number of that shade with which it corresponds in depth of colour is entered in the record as the representative of the amount of ozone in the atmosphere.

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* Abstract of a paper read to the Scottish Meteorological Society in January 1860.

Either this, or that modification of the plan suggested by Dr Moffat, is the one now actually employed by meteorologists. I have never myself worked with Moffat's ozone papers, but Dr Baker's researches prove that they excel Schönbein's in delicacy. They are not, however, altogether protected from the fallacies to which I am about to point—the objections which hold against the one, holding in some measure, if not equally, against the other.

An ozonometer was exhibited at the Leeds Meeting of the British Association by Dr Lankester; but with the construction and nature of this I am not accurately acquainted, nor am I aware that it has been practically adopted at any of the stations either of the British or of the Scottish Meteorological Societies. I am led to understand that in Scotland Schönbein's papers are those generally used; and this is the method which I conceive to be open to such serious objections, as to render all but useless the recording of observations founded on it.

Let me suppose that a bit of this prepared paper is exposed during a perfect calm. It will then be acted on only by the ozone in the air immediately surrounding it. This may be far too small in quantity to set free an amount of iodine sufficient to give an appreciable indication with the starch; *or* it may happen to be in such abundance as just to give a trace of discoloration. Let me now suppose another similar bit of paper exposed to the same atmosphere—that is, to an atmosphere containing exactly the same percentage of ozone, but which, instead of being calm, is now in a state of rapid motion —in other words, a strong wind is blowing. The action does not now cease when the ozone of the small atmosphere immediately surrounding the paper is exhausted. On the contrary, a continuous stream of air, carrying with it the reagent, flows over the paper—fresh ozone arriving at each instant to add to and increase the indication, by setting more and more of the iodine free. The paper is then examined, and No. 9 or No. 10 entered in the table as the representative of the amount of ozone in the atmosphere.

Now in both of these cases the percentage of ozone is exactly the same, yet in one case the test shows 0, or a trace, and in the other 10, or a maximum. It cannot, therefore, be taken as a faithful measurer of the varying proportions of this substance in the air, nor even as one which approximatively speaks the truth, since it is capable of so large an error.

This is the first and most important objection which I have to adduce.

If it were wished to determine the varying amount of carbonic acid in the atmosphere, and a solution of caustic baryta were resolved on as the agent for fixing it, would it be considered a matter of indifference whether the baryta were acted on solely by the carbonic acid in the stagnant air filling the jar in which it was contained, or whether a current of air were forced through the solution, leaving its carbonic acid as it passed, to add to the indication? It certainly would not; yet the ozonometer which we use is in its conditions and working all but a parallel.

My attention was first drawn to this error during the course of some investigations into the climate of North Africa, which I made in 1855. I then observed that I had always a deeper discoloration when the wind was strong, and the instrument became rather an anemometer than an ozonometer. It has, indeed, been suggested to me, that the ozone may exist in the air in a small but constant quantity like carbonic acid, and that therefore everything may depend on the velocity of the wind.

I have frequently, while walking along the shore, attached a piece of the test-paper to my hat, so that it was freely offered to the wind, and, if this were strong, in the course of an hour or two I have had a high indication, while another bit, so attached as to be protected from the wind by the body of the hat, remained unaffected, or nearly so.

In like manner, when we had a strong wind, I have exposed one paper fully to its influence; a second in the open air, but sheltered from the wind by, or under the lee of, a wall, a piece of wood, or other object; a third opposite an open window, but fairly within the room; and a fourth in a room with closed doors and windows. On the first I usually had a deep tint, on the second occasionally but rarely a trace, and on the third and fourth I never found even this.

In repeating these experiments, I varied the circumstances from time to time in order to give the results additional weight. The issue, however, was uniform—the paper sheltered from the wind was always less deeply coloured than that exposed to it, and the difference was in proportion to the completeness of the shelter.

This influence of the wind has been pointed out by other observers, but I am not aware that its full importance as a source of error has ever been indicated. When alluded to, it has not been regarded simply as a question of velocity of wind, but generally other elements, such as direction, humidity, &c., have been introduced, without notice of the very great preponderance of the velocity of the wind over all other influences.

In March 1857, Dr G. S. Thomson of Glasgow makes this observation:—" On most of the occasions on which I have detected ozone in the air, the wind was blowing pretty strongly."

In the remarks which follow Mr Forbes of Culloden's observations during the gale of Nov. 1857, we find the following:—" During this *storm, much* ozone seemed to be present in the atmosphere; but for some weeks previous to this date the air was *unusually calm, and very little ozone* could be detected by the ozone papers."

It is by no means impossible, that during the whole of the period indicated (both of storm and calm) the proportion of ozone in the atmosphere never varied.

In the Society's Report for the quarter ending March 1856, Dr Stark throws some doubt on the value of the test employed, fearing that it may be affected by other atmospheric agencies, and, at the same time, he cites some experiments made by Dr Rankin of Auchengray, who suspended papers in three situations—" *1st*, Exposed to light and air ; *2d*, Exposed to air, but shaded from light; and, *3d*, in a well ventilated apartment," and who states, " that as a general rule, he found that no change of colour was induced on the papers in the ventilated apartments or in those in the open air, *when the air was calm;* that wind, when combined with dampness, caused a deep full colour in the papers exposed to the air, but shaded from the light; that the colour was less deep in the paper exposed to light; and that no change of colour occurred in the papers suspended in the room."

This is in accordance with what I have observed myself; but I think Dr Rankin is wrong if he attributes much value to the hygrometric condition of the atmosphere, or to the action of light.

With regard to the latter, I sealed some of the papers in a glass tube, and exposed them for months to the influence of an African sun, and no change followed. The same negative result followed a similar exposure of some of the solution of the starch and iodide of potassium.

To the hygrometric condition of the atmosphere so much importance has been attached, that Dr Stark, in his very able address to this Society in January 1858, expressed a doubt whether the test at present in use indicated anything more than the quantity of *free humidity.* In this I think he is in error. When the free humidity was large, and the air calm, I was accustomed to get low indications, or none at all; while, on the other hand, with little or no free humidity, but a strong wind, I had high indications.

I do not, however, mean to say that free humidity in the air, the direction of the wind and the objects it passes over in its course, the falling of snow, the altitude of the station, and such like things, may not positively influence the amount of ozone in the air; but while the instrument for detecting the variations in this amount is so strongly affected by the mere motion of the air, that a bare trace may be marked down as 10, and a comparatively large proportion as 1 or 2—I say, while we use such an instrument, we must be very careful that the mere motion of the air is not doing what we are placing to the credit of other agencies.

In Mr Glaisher's Report to the Board of Health on the Meteorology of London in relation to the Cholera Epidemic of 1853–4, ozone receives that share of attention which it undoubtedly deserves. Both Moffat's and Schönbein's papers were used, but Mr Glaisher bases his results on those of Dr Moffat.

We are told in this Report that, "from August 24 till September 4 there was no ozone at any station near the metropolis, and very little at any station over the country. A little was shown on September 5, and from this time afterwards was exhibited generally. It was most abundant on September 24; October 7, 8, 11, 18, 25; November 19, 20, 24, 25, and 26."

Now, on examining the tables showing the daily horizontal movement and direction of the air at the Royal Observatory, Greenwich (and this may be taken approximatively as the movement of the air over all London), we find, that with one exception (the first), all those days when there was no ozone are marked *calm*, and that, while the average daily movement of the wind for August and September is 78 miles, during these days it did not exceed 41; and Mr Glaisher himself speaks of the period ending September 11 as "almost calm weather."

Turning, again, to the days when a maximum indication of ozone occurred, we find that the "whole or part" of only *one* of these days is marked "calm;" and curiously, on *that* day the air had, nevertheless, more than its average horizontal movement, which, for the whole months of September, October, and November, was 82 miles. For the special days, however (omitting the last, which appears exceptional), the mean movement was 119 miles, or three times that when there was no ozone.

He also publishes a table, showing the ozone observations at the different stations in the metropolis; and from this it appears, that at places of high elevation ozone was observed at nearly all times, while at stations of low elevation its quantity was always insignificant, and at some of them no trace of it was ever detected during the whole period of observation. He infers from this that "the presence and amount of ozone would seem to be graduated by the elevation, and to increase as we ascend." At the country stations the same thing was observed. "At all times," he says, "the amount of ozone was greatest at places of the highest elevation."

Now, with reference to the force of the wind at places of low and high elevation in the metropolis, we find it was much less over the low ones than over the high and outlying ones—the ratio of estimated force being as 2½ to 1. Even during the windy period that followed the almost calm weather ending September 11, while the average pressure on the square foot at the one set of stations was 1 lb. 7. oz., at the other it was only ¼ of a lb. And Mr Glaisher infers from this, that for the greater number of the hours of the night the air must have been "in an absolutely calm state."

It thus appears, that by mere movement of the air we have satisfactorily explained the origin of the absence of ozone on the days specified, its large presence on other days specified, and its general deficiency in low-lying places as compared with places of high elevation.

With reference to London, he accounts for its absence at low elevations "by the great amount of organic matter in the atmosphere in low districts, especially in those situated on the level of the Thames." And I do not doubt that this *is* an agency; but will it explain the same deficiency occurring in country stations? I do not think it will; nor do I regard it, either in town or country, as that which has the greatest share in giving these results. Mr Glaisher himself observes, that these low-lying stations "are also distinguished by a stagnancy of the atmosphere, and that it remains to be proved whether the total defect of ozone at all these stations is caused by the presence of large quantities of organic matter decomposed by ozone, itself being simultaneously destroyed—or whether it is owing to the small amount of ozone contained in a small volume of air, which, to obtain a perceptible elimination of iodine, should pass the test-papers in larger quantities."

For myself, I believe the *total* defect depends on neither cause alone, but probably on the two. On one—the stagnation of the air—I regard it as positive that it depends in part, and as more than probable that this part is a very large one. What is the extent of the operation of the other cause it is difficult to say, except that on the whole it must be small, since Dr Moffat tells us, in a letter to Mr Glaisher, that he has "often placed test-papers in a position exposed to the action of decaying matter, and has never seen any difference between them and others placed beyond its influence." Yet from some cause or other, we are able to infer, even from this imperfect test, that ozone is defective in and about large cities. In 1853–4 it was so at all the metropolitan stations, even at Highgate and Bexley Heath, compared with stations of the same elevation in the country. In Greenock and Glasgow, also, it is found steadily deficient. At Birmingham it was observed that there was always less ozone when the wind crossed the town or colliery district before reaching the paper; and in 1857 Dr Conway made a similar observation in London. And Dr Tripe, in the same year, observed that with north-east winds he had always less ozone than at other times, and he concluded that the air was deozonised by passing over London. Dr Moffat, too, was led to the conclusion that the products of combustion destroyed ozone.

Mr Glaisher also remarks, when speaking of the observations made in Hospitals, "that every test-paper has remained colourless which has been placed in stagnant air, whether enclosed or not, with the exception of the few cases noted at St Mary's."

At one of our own stations—viz. Smeaton, the hemispherical cup wind-gauge has lately been used for measuring the wind's motion. At the same station ozone observations are regularly made. I have had the opportunity of comparing and analysing these for the months of August and September of the past year, and with these results:—Taking all ozone observations for August below 3, we have an average of 2·8, and an average motion of the air of 70 miles. Again, taking all above 4 (going up to 7·5), we have an average of 5·2, and an average motion of the air of 142 miles; or, with about twice the depth of indication, we have twice the motion.

September, the following month, shows the same result, but not so strikingly.

I hardly think more can be required to show the existence, extent, and importance of this fallacy in the mode of taking ozone observations hitherto adopted. The remedy for it in theory is the provision of some method by which a known quantity of air, at a fixed rate, shall pass over the test-paper for each observation. Or, if the horizontal movement of the air were known during the period of the paper's exposure, from this and the depth of the tint a formula might be constructed, which would give an approximation to truth.

I made an effort to accomplish the first remedy myself. I constructed a small chamber with a diaphragm, having an aperture, into which was fitted, for each experiment, a bit of the prepared paper. The air was then drawn through the apparatus by means of an aspirator, and the rate and quantity were the same for each observation. The quantity which I was able to draw through, however, was far too small to give any indication, unless by constantly readjusting the aspirator, which involved the waiting on of the observer, and became tedious and unsatisfactory. It would not be difficult, however, to provide a large aspirator, or other mechanism, which would meet this difficulty.

Instead of paper, I would suggest the use of prepared tarlatan muslin for such an instrument. After washing this in distilled water, dip it into the solution, stretch it on a frame, and, while it is drying, tap the edge of the frame constantly on some solid body, so as to make it uniform in quality, and keep the meshes open for the transit of the air.

The railway might be made the means of determining the influence of the motion of the air over the paper, in order to calculate a formula for correction of the readings as at present carried out, which then should be recorded thus $\frac{4}{\text{S}40}$, meaning that 40 miles of south wind gave a tint corresponding to No. 4 of the scale.

But even if this source of error were fully met, there remains a second of no small importance. It often happened that on comparing the tint of the test-paper with the scale, I myself assigned to it one number, while another person placed it a degree higher, and a third a degree lower, and occasionally even a greater diversity than this occurred, amounting to 30 per cent. If the tints on the scale itself be numbered on the back, and then cut out of the slip, leaving no white margin, and a dozen persons be asked to arrange them in the order of their depth, it will seldom happen that more than six will be correct, and sometimes extraordinary errors will occur. I have tried this over and over again. When the tone of the blue or purple of the test-paper is not identical with that on the scale, as nearly always happens, it is clear that the difficulty of determining with which of the numbers on the scale it corresponds in depth of shade is still further increased. Moreover, it is constantly happening that the paper is streaked, spotted, or discoloured only at its edges, the same paper having, at some point or other of its surface, all possible shades of blue or purple, and then comes a further difficulty in assigning to it a proper number.

It will be observed that this second source of error admits of the same observation being recorded by one man as 2, and by another as 5. If our thermometers equally wanted precision, or were as difficult to read, one man might be entering the temperature at 40°, and another at 60°, at the same place and hour, and from the same instrument.

The third objection applies to the reagent itself. Dr Andrews of Belfast tells us that "oxygen may be completely converted into ozone in the presence of iodide of potassium solution." And Cloez asserts that it may give the same reactions as with ozone when exposed to the vapours of NO_5, or to the emanations from resinous trees or aromatic plants. And, according to Scoutetin, ammonia prevents the action of the ozonometer; while Dr Moffat himself, the most enlightened and indefatigable of all ozone observers, tells us that he has observed test-papers remaining for weeks in a new ozone test-box without coloration, while papers in an adjacent box were indicating 3, 6, or 8 daily, and this discrepancy he attributed to the newly wrought wood. When such slight causes become serious disturbers of the working of the instrument, and when the causes of disturbance are so numerous and so varied in their nature, there is a difficulty in deciding that they have been absent in any case.

It will not be easy, however, to discover a new and better reagent. The alcoholic solution of *Boletus luridus*, a species of fungus, which is colourless in itself, is said to become blue under the influence of ozone; the same is also true of the alcoholic solution of another fungus, the *Agaricus sanguineus*. And the white precipitated tincture of guaiacum is also rendered blue by ozonides, and I think by free ozone. Perhaps, therefore, some other colour test may be discovered, whose action will not be so easily disturbed. I have none, however, to suggest.

The fourth objection arises from the curious fact, that a paper exposed at night may at six or seven in the morning be deeply discoloured, and at nine or ten, the regular hour for the reading, be bleached again. This occurs independently of light or moisture in the air. Its cause I am not able definitely to assign. But as a source of error it is of great gravity. It has been asked whether it may not depend on the action of antozone; but the existence of this body in a free state has not yet been determined. The most probable explanation, I think, will be found in the volatility of the liberated iodine, which does not combine with the starch to form the fixed iodide of starch till the paper is moistened.

Ozone being such a powerful oxidising agent, it was generally expected that its varying proportions in the atmosphere would be found to have a direct and marked influence on the prevalence or rarity of certain diseases. This I think was a reasonable expectation, and the introduction into the study of climate of this chemical element was rightly regarded as full of the promise of good results. That this expectation has hitherto met with a signal disappointment, no one, I conceive, can wonder; because, while we have been assuming that we were measuring and recording the varying proportions of this substance, we have in reality been doing no such thing. I have shown that the rate of the atmosphere's motion *must* affect every observation, that the reading of the instrument admits of wide error, and is always uncertain; and that the sources of disturbance from causes of a local character are numerous and very strong. No sound conclusions, indeed, can be deduced from the comparison of the fluctuations of disease with ozone observations as at present made.

It may be said, however, that it is of importance to know what quantity of ozone is brought to act upon man, and not what percentage the air contains, since it is that which will affect him, either directly or through his surroundings. And in this there is such force that I would not have the consideration overlooked. It bears not only on ozone, but on many other elements of climate, and, I repeat, is one of practical importance. But if such a view be adopted, we should then require an instrument which will fix and measure all the ozone which passes over a given surface during a given period. The present method does not and is not intended to do this.

I trust that these remarks may not lead to hopelessness in any observer. "The whole investigation is still within the region of experiment," and my aim has been to point out certain errors in that mode of conducting the inquiry, which we at present follow with a seeming belief in its trustworthiness. To know that our course is wrong, is the first thing to lead to a search after, and the discovery of, the right and true one. It is this, and not a hopeless pause, which I desire to see as the result.

Mr Glaisher, in his Report to the Board of Health, of which I have spoken, says in reference to ozone, "I rejoice that the persevering spirit of inquiry which distinguishes the present age should have added another meteorological element of investigation, and one too which, if somewhat verging on the field of chemical inquiry, promises to be a subtle and important agent in aid of this research into the nature and extent of meteorological influences upon the rise and progress *of cholera*," and we may add *of disease* generally. In this I heartily concur, remembering that Sydenham long ago pointed out, that "years which coincide in their appreciable atmospheric characters differ in the diseases by which they are infested, and *vice versa;*" and that "there are different constitutions in different years, which originate neither in their heat nor their cold, their wet nor their drought, but depend upon certain hidden and inexplicable changes." Is it not possible, that in ozone may be found that "less manifest than occult condition," of which the Father of British Medicine was here dreaming?

Mr Burgess, the Secretary of the Society, has suggested a mode of constructing a table, by which the percentage of ozone might be found from the two data—the indication on the paper and the wind force.

He supposes that the extreme velocity of the wind may intensify the indication of ozone by 5 degrees of its scale (an estimate he believes to be under the truth); he then supposes that 6 grades of wind force may be distinguished, and that the augmentation of ozone indication by each of these is nearly the same (though experiment may show the ratio of increase to be more complex); then a table might be constructed on the plan of that here given, from which we may register the *percentage* of ozone existing in the atmosphere.

4	3	2·5	2	1·5	1	0
I.	2	3	4	5	6	7
II.	3	4	5	6	7	8
III.	4	5	6	7	8	9
IV.	5	6	7	8	9	10
V.	6	7	8	9	10	11
VI.	7	8	9	10	11	12
VII.	8	9	10	11	12	13
VIII.	9	10	11	12	13	14
IX.	10	11	12	13	14	15
X.	11	12	13	14	15	16

The numbers in the upper line are the relative velocities of the wind, and the first vertical column contains the numbers of the ozone scale; the others are the numbers to be registered as the percentage of ozone in the atmosphere. Thus if, after an exposure for a definite time, the ozone paper shows 7 of its scale, and if the mean velocity of the wind for the same time is 2·5 of the wind scale, the entry of ozone to be made in the observer's journal is 9, whereas, had the wind been calm, it would have been 13. This also implies that 1 on the ozone paper in calm air shows the same *percentage* of ozone as 6 does when the wind blows strongly. From the results of experiments a table *of this form* might be correctly constructed.

Contributions to Microscopical Analysis. No. 2. Celastrus scandens, Linn., *with Remarks on the Colouring Matters of Plants.* By GEORGE LAWSON, Ph.D., Professor of Chemistry and Natural History in the University of Queen's College, Canada.*

Two years ago, viz. on the 10th December 1857, I read to the Botanical Society the first of a series of papers on the Microscopical Characters of Vegetable Substances. My intention was to bring before the Society, from time to time, such of the more important drugs and articles of food as had escaped the notice of other histologists, as well as to supply from observations such omissions, and correct such inaccuracies as seemed to obscure previously published descriptions. My departure from Edinburgh interrupted the continuation of the series. I now propose to resume it. Instead, however, of selecting substances from the British markets and the Botanical Museum, I shall rather give the results of a histological examination of some of our more important Canadian plants, especially those species whose properties render them useful in medicine or the arts.†

On the present occasion, I select for remark one of our most ornamental Canadian plants, *Celastrus scandens*, Linn., chiefly on account of an interesting structure observable in the colour-substance of the arillus, which presents a very distinctive histological character. This is one of the most conspicuous plants which attract the attention of autumn visitors to the

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* Read to the Botanical Society of Edinburgh, 8th March 1860.

† An abstract of my former paper on this subject will be found in "Trans. Bot. Soc. Edin." vol. vi. p. 25; also in the "Edin. New Phil. Jour." vol. vii., new series, p. 315.

Falls of Niagara; for although its flowers are small and greenish, the fruit-clusters are large, and showy in their colouring. The fruit is globose, of a very bright orange colour, and opens (usually by three, sometimes two or four valves) to display a deep scarlet-coloured pulpy mass, consisting of the arilli, in which the large brown seeds are enveloped.

There is a good description and figure of the plant in Gray's "Illustrated Genera of United States Plants," vol. ii. p. 185, plate 170. See also "Persoon, Synops.," i. p. 242; "Hook. Fl. Bor. Am.," vol. i. p. 120; "Torrey and Gray, Fl. N. Am.," vol. i. p. 257; and "Gray's Man. Bot. N. States," p. 81. The plant goes under the common names of climbing staff-tree, Virginian wax-work, and shrubby bittersweet. It was introduced into England from America, so long ago as the year 1736, by Peter Collinson (Col. MSS. in "Hort. Kewensis," 2d edit. vol. ii. p. 26).

In regard to its properties, the seeds of this species are said to be narcotic and stimulating (Gray, *l. c.*); and most authors agree in attributing emetic and purgative properties to the bark (See "Balfour's Class Book," p. 794). It must be confessed, however, that a certain amount of dubiety attends some of the published statements of the properties attributed to this plant. The fullest indication which I meet with is perhaps that contained in few words in "Lindley's Vegetable Kingdom," 3d edit. p. 587, and that indication is by no means so satisfactory as could be wished. After observing that the seeds of the European species of *Euonymus* (which also belong to *Celastraceæ*) are nauseous, and said to be purgative and emetic, that sheep are said to poisoned by them, and that an ointment was formerly prepared from them for the destruction of pediculi in the head, Dr Lindley proceeds to state, that "similar qualities have been found in the bark of *Celastrus scandens*," &c.

We know that many plants of the order *Celastraceæ* are by no means inactive, and this circumstance ought to induce a more careful examination of the properties of the one before us. Royle states that an active principle has been detected among the Indian species, which acts with more or less activity; several yield an oil which is used in burning. *C. paniculata*, Willd. (*Malkungnee*), yields oil which is both useful for burning and in medicine. It is highly valued by the native practitioners in India, and is employed in the disease called *Berriberri;* it is described as having a bitter and acrid taste. I find oil in the cells of the copious albumen of the seeds of *C. scandens.* The beautiful light-yellow inner bark of *Euonymus tingens* is said to be useful in diseases of the eye, and to be used also to mark the tika on the forehead of Hindoos; its use as a dye has been suggested. I also find it mentioned by Lindley and others, that the leaves of *Catha edulis,* the kat or khât of the Arabs, have stimulant properties, which, it is said, cause in those who eat them extreme watchfulness, so that a man may stand sentry all night long without drowsiness. The Arabs are fond of these leaves, and have faith in them as an antidote to the plague; Botta says that, when fresh, they are very intoxicating. The species of *Elæodendron* yield a drupaceous fruit, which is in some cases eatable, as in *E. Kubu,* whose drupes are so used by the colonists of the Cape of Good Hope, while the fresh bark of the root of *E. Roxburghii,* rubbed with water, is employed by the natives of India to allay swellings; it is, says Roxburgh, a very strong astringent. The wood of plants belonging to this order also has its special uses. That of *Celastrus serratus* is charred in Abyssinia for cannon gunpowder (Richards), whilst that of *Euonymus Europæus* is used for the same purpose in France; the charred young shoots find a more peaceful application in the artist's hands.

As I have already indicated, the chief point of interest in the histological structure of *Celastrus scandens* is found in the tissue of the arillus, which presents a kind of colouring matter of which we as yet know but few examples in the vegetable kingdom. That we may fully appreciate this peculiarity, let us, in the first place, refer to the conditions under which colouring matters usually exist in plants. The green colouring matter (chlorophyll) of leaves and other green parts, occurs almost, if not quite, constantly in the form of insoluble granules, which are usually well defined, and most commonly more or less globose. The yellow colouring matter of flowers also frequently exists in the form of granules, but often also dissolved in the cell-sap. The red and blue colouring matters on the other hand are almost always diffused in a state of solution in the cell-sap. But while all this is true as a rule, exceptions occasionally present themselves. If, according to Mohl, the reported presence of green-coloured cell-sap in plants is due merely to imperfect observation, we at least know that amorphous chlorophyll does occur, and it likewise seems probable that in many cases the apparently large and well-defined granular aspect of chlorophyll is dependent upon the association with it of starch granules. Exceptions to the usual character of the red and blue colouring matter in flowers occur in the case of *Salvia splendens* and *Strelitzia Reginæ.** In the latter plant, both the blue and yellow colouring matters are peculiarly interesting, of which I recollect to have exhibited preparations to the Botanical Society some seven or eight years ago. This is one of the few plants in which we have striking examples of the occurrence of colours belonging to both the xanthic and cyanic series in one flower. We have here a peculiarly rich blue, or violet blue, associated with a peculiarly rich orange-yellow. But it is the histological condition of the colouring matter that I wish especially to refer to. In the blue parts of the perigone, the colouring matter in the cells consists entirely of spherical granules of an intense blue or violet-blue colour; occasional cells contain similar shaped granules of bright crimson or rose-colour. The granular character of the colouring matter is quite constant, but is apt to be overlooked if the fully expanded or old flowers are selected for examination. It is best seen in the tissue, long before the expansion of the flower. The spathe containing flower-*buds* should therefore be cut open so soon as it is perceived rising up from the rhizome, at the base of the leaf-stalks; even at that stage, before they have been exposed to light, the parts of the perigone will be found to have acquired their deep golden and azure hues (although they are not so brilliant as they afterwards become), and the globular granules are seen distinctly in the cells, the edges of which present the peculiar crumpled appearance so common in the colour-cells of flowers. When the flower attains maturity, the cell usually becomes completely filled up with granules, which then present the aspect of a dense homogeneous mass of opaque blue matter, in which the granular character cannot be distinctly seen: this effect is heightened when some of the granules become broken up. Even in the mature flower, however, some cells may usually be found containing fewer granules, and in these the granular character of their contents may be observed without difficulty.

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* See "Mohl's Principles of the Anatomy and Physiology of the Vegetable Cell," Henfrey's Edition, page 44.

In the yellow parts of the perigone, instead of spherical granules, we find the colouring matter in the form of *filaments*, which are spirally twisted and rolled up in various ways in the cell, resembling, to some extent, in their twisting the delicate spiral fibres in the cells of the aërial roots of epiphytal orchids. But the fibres are in many cases very short, and form small round coils, which, under a low power of the microscope, give the outline of globular bodies, resembling the nuclei of many plants.

To these examples of abnormal colouring matter I have now to add the pulpy arillus of *Celastrus scandens*, which, although of a different colour, strongly reminds me, in its histological characters, of the yellow parts of the perigone of *Strelitzia*. In the *Celastrus* the arillus consists of more or less elongated oblong cells, whose membranes are colourless and quite transparent. The colouring matter is contained in these cells, and presents itself in the form of numerous minute elongated granules, exhibiting well-defined outlines; they present considerable uniformity both in size and shape. They are slender, between linear and laneolate in form, and either acute or more or less acuminate at both ends. They are for the most part straight, but in some cases curved, more or less perfectly, into a crescent, and, more rarely, the curvature is so great that the two extremities meet, and then the granule assumes the form of a ring. These granules are all of a uniform bright scarlet colour. They are frequently aggregated in masses in the cells. Usually, however, they are quite separate, and lie without order in the cell, crossing each other in various directions. In narrow, elongated cells, whose diameter is less than the length of the contained granules, the latter are usually arranged conformably, lying in the longitudinal direction of the cell, just as we find in the case of the raphides which occur in elongated special cells in endogenous plants. On an average, the colour granules measure the two-thousandth part of an inch in length, by the twelve-thousandth part of an inch in breadth, being thus about six times as long as broad. The cells in which the granules are contained are somewhat variable both in size and form. When quadrangular, and not much longer than broad, they measure about the six hundred and seventieth part of an inch in diameter; but in cases where they are much elongated, their breadth is proportionately decreased, as is usually the case in parenchymatous tissues.

Under a low power (as, for example, the lowest power of Nachet's microscope), the seed of *Celastrus scandens* also presents a characteristic aspect in its minutely embossed surface, of a fine fawn colour. On increasing the power, it is found that the embossed appearance is dependent upon the slightly elevated surfaces of the cells, which are compact, regular-sized, and sufficiently distinctive of themselves to lead to the discrimination of the tissue from most other vegetable tissues. A cross section of the membrane forming the seed-covering (readily obtained by making a thin horizontal slice of the whole seed), presents a peculiar striated appearance, which is also characteristic enough. The cells of the albumen contain oil.

My remarks on the colouring matters of plants have extended to a greater length than I anticipated; but that they are not foreign to the main subject, viz. the Microscopical Analysis of Commercial Substances, will appear in the course of succeeding papers. Our knowledge of the chemistry of these colouring matters is still very imperfect. Chlorophyll is not known in a state of purity, and the changes of colour which it undergoes have been only partially explained. By Fremy and Cloez the colouring matters of flowers are referred to three distinct substances, two of which are yellow, while the other is of a blue or rose colour. The blue or rose colour is produced by a compound which has been termed *Cyanine,* the blue tint becoming red when exposed to the action of an acid.

The yellow matter, which is insoluble in water, is termed *xanthine*, and that which is soluble has received the name of *xantheine*. These bodies, however, have not been isolated in a pure condition; and some of the facts above recorded indicate at least a probability that three such bodies are insufficient to account for all the observed phenomena of flower-colouring.

On Sarcina ventriculi, Goodsir. By JOHN LOWE, M.D., Edinburgh.*

The discovery of *Sarcina* by Professor Goodsir, in the frothy vomit occasionally met with in severe cases of stomach disease, has given rise, at one time or other, to no little conjecture: 1*st*, As to its real nature; 2*d*, As to its source; 3*d*, As to its pathological relation to the affection in which it is found to occur; and 4*th*, As to the reason of its continuing to flourish in a locality so evidently unfavourable to the development and nutrition of a vegetable organism.

That the structure is of a vegetable nature was clearly shown by the discoverer—its peculiar form and fissiparous mode of propagation, and its action under re-agents, clearly set this point at rest; but then came the question, To what group does it belong? Its general resemblance to the *Desmideæ*, and its quaternate arrangement of parts, caused it to be ranked amongst the *algæ*.

So far the subject advanced, but it still remained to discover the source from which the plant had its origin.

Is it taken into the stomach with the food, and, if so, with what part of it? The solids or fluids? That it obtained entrance with one or other of these seemed probable.

At one time I discovered in some stagnant water the counterpart of *Sarcina ventriculi*, and imagined that I held the key to the problem. This idea I relinquished on recollecting that it had been found in the kidney and lung as well as in the stomach. Since that time, further investigation has proved to me that there is nothing improbable in the supposition that it is occasionally imbibed with water into the stomach, having existed in the algal form in that fluid. Its occurrence in the lung and kidney I shall attempt to account for presently.

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* Read to the Botanical Society, 12th April 1860.

The merit of first suggesting the actual origin of sarcina is due to Mr Berkeley, who stated in the "Gardener's Chronicle" (1857, Aug. 29), that he had made experiments to prove that it belonged to one of the common fungi, Penicillium or Aspergillus. This he was unable to do; but the discovery by Mr H. O. Stephens of quaternate cells on a yellow fungus found growing on bones, rendered it highly probable that the view was a correct one.

In a communication read before the Botanical Society in 1857, I showed that parasitic fungi were derived from the two above-mentioned genera; and singularly enough, following upon that we have the discovery, by Dr Tilbury Fox,* of sarcina in a case of parasitic skin-disease. Then, in September of the present year, I found most perfect specimens of sarcina in a phial in which I had some months previously placed a quantity of crystals of cholesterine obtained from a hydrocele.

We have thus acquired a series of links in the chain of evidence towards establishing the truth of Mr Berkeley's surmise, which, if not amounting to positive demonstration, is nevertheless so strong as to leave little doubt of the accuracy and justice of that gentleman's observation.

The fact is not a little interesting, inasmuch as we have now very good grounds for believing that there is no fungus which infests the human body, nor, I believe, any animal body, which is not referrible to one of the common genera, Penicillium, Aspergillus, and Mucor.

We may now consider by what means sarcina obtains ingress to the lungs and kidneys. There can be little doubt that the spores of the fungi above named are carried into the pulmonary passages during inspiration and there undergo development, and that according to various modifying agencies they give rise to a mycelium which may or may not produce aërial fructification. Of the conditions which seem to be requisite for the production of sarcina, I shall speak by and bye. To account for its occurrence in the kidney we must look for another mode of conveyance, as it is scarcely probable that the spores of a fungus could enter the bladder and pass along the ureters; for to effect this against the stream of urine, presupposes a locomotive power either in the spore or in the epithetial lining of the passages. We do not find either the one or the other. We must then believe that the fungus finds entrance through the circulating system, and this I regard as neither impossible nor improbable; but we have to inquire, in the first place, how the fungus obtains admission into the circulation, for it is evident that this cannot be effected in the form of sarcina, nor yet as the spore, both of which have a diameter as great, or greater, than that of blood-cells. I shall then briefly notice the means by which I think this is brought about, reserving a more extended notice which so important a subject deserves for a future occasion, after a more careful observation and investigation of facts. About two years ago, whilst examining some specimens of fungi which I had undergoing development, I found one which presented an hitherto unknown appearance. On the cork of a phial containing some ropy mushroom catsup, I observed a number of globular yellowish-white bodies about the size of pins' heads. Placing one of them under the microscope, I was surprised to find that it consisted of an innumerable quantity of non-nucleated cellules, most of which had a diameter of 7000th to 10,000th of an inch; some few being twice as large. The most minute search failed to render apparent anything like a common investing membrane. On examining the fluid in the bottle, it was found to contain a vast number of similar bodies in various stages of development. Thus, while the majority bore the same features as those on the cork, others were found to be considerably enlarged, and contained a nucleus; others, again, had assumed an oval form, and had begun to form gemmæ; whilst some had already acquired a distinctly tubular or mycelial aspect. The smallest of these cellules exactly resembled the nuclei of old yeast-cells, or what are termed by Turpin "*Globulins seminifères,*" which are found in such abundance in beer at the commencement of fermentation.

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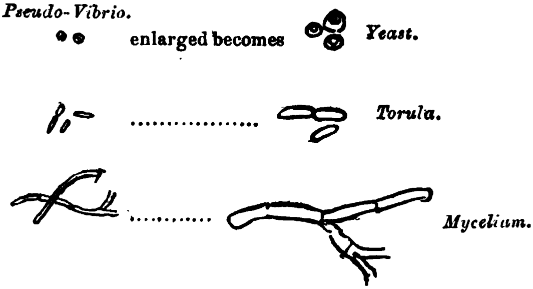
* On the Identity of Parasitic Diseases, &c.—*Lancet*, September 10, 1859.

I have little doubt, indeed, that they have their origin from the liberated nuclei of common fungi, capable, under certain conditions, I believe, of undergoing division indefinitely, and of retaining the same form, but reverting to their original conformation so soon as they are placed in suitable pabula. There is nothing very improbable in this supposition, when we consider that yeast is also propagated indefinitely by gemmation and nucleation, retaining the form of yeast only whilst it remains in a saccharine fluid, but advancing to the stage of mycelium whenever the sugar is exhausted. The first change noticed from the globular form is to the oval, then to the filamentous condition. Now, just the same changes are to be observed in the progress of the cellules under consideration; they are first spherical, then oval, then linear. Increase in magnitude may go on at any of these stages. Thus the spherical may, with proper food, revert to the condition of yeast, the oval to that of the torula, the linear to that of the mycelium (*figures*). It will be remembered that these cellules were first found on a cork, where they had doubtless taken their rise from a single nucleus. They are capable, then, of growing aerially, and thus, from their minute form, can be wafted into the air in myriads. If we examine the white powder found on old beer-barrels and on wooden utensils, wherever decaying organic matter is present, we shall find that it



consists entirely of these minute bodies, which have been frequently noticed and figured as Vibriones, but which in reality are of vegetable origin. Their diameter, as before mentioned, is about $\frac{1}{10,000}$th of an inch, and some even smaller than this—not small enough to pass through a membrane, but finding access probably through slight lesions of the capillaries or veins of the mucous surfaces. Whether this hypothesis will be found to hold good remains to be proved. I merely throw out the suggestion as one most likely to yield important results. The importance of the subject, indeed, is one which cannot be over-estimated; for if we reflect that myriads upon myriads of these minute objects are constantly floating about in the atmosphere; that they are capable of entering through the finest conceivable apertures; that their agency is purely zymotic; that bodies very closely resembling these, if not identical with them, have been found in the blood and kidneys of patients affected with typhus; if, I say, we bear in mind these facts, we must admit that there is still a great deal to be learned before we can be said to know the entire history of these apparently trivial agents.

Whether they enter the body by the channels I have pointed out, or whether by the most improbable route of the ureters, I regard it as most likely that these give rise to sarcina in the kidney; and it appears to me far from unreasonable to suppose that various zymotic diseases, if not originated, may be accelerated by the presence of these minute cellules in the blood.

Having considered the nature and origin of sarcina, we may say a few words about its relation to the disease in which it occurs. Is it merely of accidental occurrence, or is it a morbific agent in the diseases in which it is found? That there must be pre-existing disease before the parasite can be developed in the stomach, is, I think, indisputable; for in the healthy stomach, the gastric juice would certainly be sufficient to destroy any such growth. When, however, the secreting power of the stomach is impaired, or in a great measure lost, by reason of ulceration, &c., then the fungus finds a nidus amongst the diseased tissues, and in all probability tends greatly to increase the irritation of the viscus;

at any rate it does so indirectly, if not by immediate contact, and this by virtue of its power of exciting fermentative decomposition, the products of which, by distending the stomach, and by their irritant action, cause frequent efforts at vomiting, and give rise to the yeasty appearance of the ejected contents.

Finally, as to the reason of the plant continuing to grow in what appears at first sight to be an unsuitable locality. This, I have already stated, is in a measure owing to a previously vitiated state of the lining membrane of the stomach; but there is evidently some special food which it meets with, and which it finds in but few other localities, serving to retain it in the state of sarcina. Indeed, I regard it as essential to its development, that this peculiar pabulum should pre-exist. In what, then, does this peculiarity consist? The specimen of cholesterine crystals in water, which gave rise to sarcina, I found to be most intolerably fetid, from the disengagement of hydrosulphuret of ammonium. Mr Stephens finds his specimen of the plant on bones. In other cases it occurs on diseased tissues, the decomposition of which would yield some such gas as the above. May not this, or a similar gas, be the food requisite for the production of this peculiar form of the plant? It seems to me to be not improbable, and that on the exhaustion of this supply it returns to its pristine form, just as yeast acts, after the failure in the supply of sugar.

However far these suggestions may be found to hold good, it admits now of scarcely a doubt that sarcina is neither more nor less than an algal condition of a common fungus. Mr Berkeley, indeed, speaks of it as being the spore of the plant. With much diffidence, I venture to express an opinion at variance with that of so excellent a mycologist.

It seems to me that the term spore is often loosely and vaguely applied to small cryptogamic cells, whose origin and purpose seem to be obscure. The term ought, I think, to be confined entirely to those bodies which are the result of a true reproductive process. There is, so far as I am aware, no observation to prove that sarcina is so produced; and we ought therefore to avoid giving it an appellation which is calculated to originate an erroneous impression of its nature. We have much to learn as yet regarding the reproduction of fungi, and it will, I believe, be found eventually, that the fact of cells undergoing segmentation is entirely opposed to the view of their being spores or true reproductive cells. Looking at it in this light, it seems quite contrary to experience, and all our ideas of sexual reproduction, to imagine that the ovum may go on dividing itself into millions of other ova, each capable of producing the mature plant. We should thus have, as in the instance of yeast, many millions, nay, even millions of millions of plants arising from a single ovum. From the analogy observable in other cryptogamous plants, we may, I think, assume it as a fact, that each true reproductive cell can give rise to only a single mature individual, but that a single plant may give rise to endless gemmations. And, as a corollary to this, I would add, that where gemmation, or, what is the same thing, fissiparous division, exists, there is no reproductive process, and *ergo*, the results are not true spores.

From this it follows, that yeast is nothing more than a gemmation of the fungus. True, it is derived from the so-called aërial spores of the penicillium, &c., but these are, I believe, in reality gemmæ, just as the *spores* of a fern are. The true reproductive organs exist in the mycelium. So with sarcina, whose fissiparous division is nothing more nor less than mere budding. And so with other fungi, which are propagated in like manner.

These views on so obscure a subject are not put forth dogmatically, but merely to excite inquiry into a subject which is surrounded by much that is interesting. That they will bear investigation, however, I fully believe, since there is no statement made which is not borne out by analogy in other cryptogamic families.

Experiments on the Effects of Narcotic and Irritant Gases on Plants. By JOHN S. LIVINGSTON, Fellow of the Royal Physical Society, Edinburgh.*

Several years ago, the effect of narcotic and irritant gases on plants was made the subject of a joint series of experiments by Dr Christison and the late Dr Turner, whose evidence was called for in a case then pending before one of our law courts, in which damages were claimed for destruction of trees and deterioration of property, said to be caused by the exhalations from a black-ash manufactory that had been established in the vicinity. The question, then, of the effects of gases on plants is of more than a purely scientific interest, and claims attention even from those who look on every scientific inquiry as valueless unless it have some immediate and obtrusive bearing on human concerns.

The experiments which I now proceed to detail are many of them repetitions of those performed by Drs Christison and Turner, with a view to test their accuracy; with this difference, that the *proportions* of the gases employed in the experiments of Christison and Turner have been purposely avoided. Some of the gases, however, have been experimented with by myself only; nor are all, or nearly all, of my experiments detailed, but only such as seemed most illustrative.

The *modus operandi*, when large quantities of the gases were to be employed, was simply to collect the gas in the usual way into stoppered bottles of known cubic capacity, and to allow it to diffuse under bell-jars covering the plants. These bell-jars were rendered perfectly air-tight, by causing their edges to rest on a bed of glazier's putty, pressing the jars down tightly, and securing against any crevice by puttying the outer edge. When the quantities to be used were small, by means of a hole bored in the table we could inject, with a graduated glass syringe, with perfect accuracy, any quantity of the gas, from four cubic inches to the $\frac{1}{20}$th of a cubic inch.

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* Abstract of a paper read to the Botanical Society, May 10, 1860. The paper was given in as an essay in the Botanical Class of the University of Edinburgh, and gained the prize offered by the Professor.

I. Sulphurous Acid.

1. A young Laburnum and Psoralea were introduced into a jar of the cubic capacity of 2000 inches, along with 4½ cubic inches of the SO_2, or in proportion of 1 to $444\frac{4}{9}$. No change was remarked until the plants had been exposed to this atmosphere for six hours, when the leaves began to shrink. They were then left overnight, and, when examined next morning, or after an exposure of twenty-two hours, the Psoralea was found to be perfectly dead, lying flat on the earth, with its leaves all shrivelled and discoloured. The Laburnum was also so much affected as to be to all appearance likewise dead; the leaves drooped, and were of a yellowish brown colour. The main stem still continued succulent to a certain extent, but the plant had been so powerfully acted on as to be beyond recovery.

2. Into a jar of 2000 inches cubic contents was introduced a young Laburnum, with the fourth of a cubic inch of the gas, or 1 in 8000. In twenty-four hours the cotyledons had become discoloured at their junction with the stem, and in forty-eight hours they were dry, shrivelled, and the leaves drooping. At the end of seventy-two hours no farther change had taken place, except that there was a slight inclination of the petiole to droop. On the fifth day of exposure the drooping had become decided, but as yet no discoloration had shown itself. On the sixth day no further change had taken place, but, on the seventh, the edges of some of the leaves had become of a fawn colour, and the leaflets had folded on themselves.

3. Another Laburnum was placed under a jar of 200 inches cubic capacity, with four-fifths of a cubic inch of SO_2, or 1 in 250. In twenty-four hours, no effect of the gas had taken place. In forty-eight hours, a slight tendency to curling of the leaflets had set in; and by the third day the leaves had drooped considerably. On the fourth day the summit leaves exhibited a decidedly withered appearance. By eight o'clock of the seventh day, the cotyledons had dropped off; and by two o'clock P.M. of the same day, the plant, in some of its leaves, became completely discoloured, and hung down as if dying. The plant was then removed, and ultimately recovered, but not without first shedding its leaves.

II. Hydrochloric Acid.

Though, as we have seen, SO_2, in very small proportions, acts powerfully as an irritant poison on plants exposed to its influence, hydrochloric acid will be found to be even more injurious.

1. A Laburnum was placed under a jar containing 2000 cubic inches of air, with 4½ cubic inches of hydrochloric acid gas, or in proportion of 1 to 444⁴⁄₉. In forty minutes, the plant had assumed a greenish gray hue. In twenty-two hours the cotyledons had become quite brown, dry, and shrivelled—the leaflets had likewise become shrivelled, and of a dark olive colour.

3. Into a jar containing 200 cubic inches of air, 24 cubic inches of HCl, or 1 in 8⅓, were introduced, along with a Balsam. In half an hour the plant had begun to droop, and exhibit discoloration on the margins and tips of the leaflets. In one hour and a half the drooping had become very considerable, and the plant had a flaccid appearance. In twenty-two hours it was quite dead, the leaves had become quite brown, and their tissue had so little tenacity as to go to pulp when handled.

3. Into a jar containing 84 cubic inches of air were introduced four-fifths of a cubic inch of HCl, or 1 in 105, along with a Psoralea. In ten minutes it had shrivelled considerably, and in one hour and a half some of the leaves had become discoloured, and the whole plant had a flaccid appearance. In twenty-two hours very many of the leaves had become half discoloured, and several wholly, while most of the petioles hung down.

4. One-fifth of a cubic inch of this gas was passed into a jar containing 2000 cubic inches of air, or 1 in 10,000, along with a Balsam. In half an hour one of the cotyledons had become discoloured on the edge, and a tendency to droop, though slight, was visible. By the time it had been exposed one hour and a half, the drooping had become most decided, and a tendency to shrivel had exhibited itself. In twenty-four hours the leaves were hanging down, and in forty-eight hours they had become brown at tips and edges, the cotyledons were dry and withered, and even the main stem drooped a little. When taken out, the cotyledons and three of the leaves

fell off. The plant was transferred to a hothouse, where it recovered, but parted with all its leaves; young ones were however soon put out. It was not a little curious to observe that many of these were withered at the tips, from the leaf, in its very young state, being subjected to the withering influence of the gas; but the plant still possessing vitality sufficient to develop the entire leaf and leaf-stalk, the traces of the violence done it in the bud continued, and would continue, to present themselves during the life of the plant.

III. Chlorine.

1. A young Laburnum was put into a jar containing 2000 cubic inches of air along with 4½ cubic inches of chlorine gas, or 1 in 444$\frac{4}{9}$. In an hour and twenty minutes a very slight tendency to browning of its leaves took place. In twenty minutes more, the tendency to discoloration had become decided. For the next few hours the gas showed its effects less rapidly, as no great increase of the discoloration took place; but in twenty-four hours the leaves had completely lost colour, and were seemingly dried up and drooping. This plant, which was also removed, as in the former cases, shed its leaves, put out new ones, and became as vigorous as ever.

2. Into a jar containing 2000 cubic inches of air another Laburnum was introduced, with 12 cubic inches of chlorine, or 1 in 166⅔. In less than an hour some of the leaves had become completely discoloured—all of them more or less so; but as yet no drooping had taken place. In less than two hours many of the leaves were quite blanched, and only one had entirely resisted the action of the gas. We observed that the blanching invariably began at the tips of the leaves, and gradually crept along to their base. By the time twenty-four hours had elapsed, the plant was completely blanched, *with the exception of the terminal leaf-bud*, which remained apparently unaffected—both in this and the preceding experiment—probably because the leaf being undeveloped, it had not begun to aid in the respiration of the plant, and so had not imbibed any of the noxious vapour. In both these experiments the stem remained green and succulent, and the plant

ultimately recovered, with only the loss of its first crop of leaves, from a violence that to all appearance seemed likely to prove fatal to it. It soon, however, put out a new and vigorous foliage.

IV. Sulphuretted Hydrogen.

1. Into a jar of the capacity of 2000 cubic inches a young Laburnum and Balsam were introduced, along with $4\frac{1}{2}$ cubic inches of sulphuretted hydrogen, or 1 in $444\frac{4}{9}$. In twenty-two hours no change of colour had ensued, but both plants were drooping—the Balsam very considerably, and the Laburnum slightly. In twenty-seven hours the drooping in the Laburnum had increased, but no change of colour had taken place; the Balsam was hanging its leaves quite perpendicularly, but, like the Laburnum, had not been in the least discoloured. The plants were removed, and at first seemed to be likely to recover, but of a sudden they drooped, and died completely down.

2. Two similar plants were introduced into a jar containing 200 cubic inches of air, along with 7 cubic inches of the gas, or 1 in $28\frac{4}{7}$. In twenty-four hours the Balsam had drooped only slightly, and the Laburnum scarcely at all. In twenty-seven hours, the Laburnum drooped not only its leaves, but even one of the petioles; but no discoloration was observed. The Balsam drooped much, some of the leaves falling off in removing it from under the bell-jar, but it was not otherwise affected, continuing as green as when introduced. This result is a curious one, as seeming to show that a large volume of the gas affects the plants, to all appearance, less than the smaller quantities.

3. Into a jar containing 130 cubic inches, a Balsam was placed, with four-fifths of a cubic inch of the gas, or 1 in $162\frac{2}{5}$. No effect was visible on the plant after exposure to its influence for twenty-four hours, but in twenty-seven hours it drooped. When removed after that time, though the plant survived, it never after seemed healthy. It may be remarked, in all the above experiments with HS, there was along the margin, and on the tips of the leaves, a copious deposition of drops of water.

V. Ammonia.

1. A Balsam was next introduced into a jar of the cubic capacity of 180 inches, along with 2 cubic inches of ammonia, or 1 in 90. In twenty six-hours the plant had drooped considerably, but not a trace of discoloration of the leaves had taken place.

2. A similar plant, placed in 85 cubic inches of air, along with one-fourth of a cubic inch of ammonia, was not affected in twenty-six hours beyond a very slight drooping. No discoloration was remarked, the plant being as green and succulent as when put in.

VI. Protoxide of Nitrogen (NO) or Nitrous Oxide.

1. Into a jar of cubic capacity 2000, was placed a Balsam, with 24 cubic inches of protoxide of nitrogen, or 1 in 83⅓. In half an hour the plant had drooped considerably. In nineteen hours the drooping had not increased, but one of the leaves had shrivelled, and a cotyledon lay on the ground. Two of the leaves had their tips covered with mould, but they were as green as at first. In forty-three hours no change seemed to have taken place, farther than that now the other cotyledon and a leaf had fallen off. In sixty-eight hours no effect was remarked beyond what had already shown itself, and the plant was removed, but rapidly died down.

2. A Balsam was introduced under a jar containing 200 cubic inches of air along with 26 cubic inches of NO, and in half an hour the plant drooped, though slightly. No increase of the drooping took place in nineteen hours; but two of the leaves were covered with mould, and were lying on the ground. The plant was allowed to remain exposed to the influence of the gas for three whole days, but showed no symptoms of having been further affected. When removed after that time, it died quickly down.

VII. Carbonic Oxide.

1. Into a jar of cubic capacity of 130 inches, a Balsam was placed, with 4½ cubic inches of CO, or 1 in 28⅝. In nineteen hours there was evident drooping and a slight shrivelling of

some of the leaves. One leaf had fallen off, while the bottom of the pot was covered with patches of mould, but no discoloration took place. The effect of the gas did not show any increase in forty-eight hours, except that now two leaves had fallen off. The plant was removed, but died rapidly down.

2. A Balsam was introduced into a jar of cubic capacity of 185 inches, with 7 cubic inches of CO, or 1 in 26$\frac{3}{7}$. In nineteen hours the plant had drooped much, and a deposit of mould had taken place in the pot. Though allowed to remain for three days, no further effect was produced, beyond the falling off of one of the leaves. The plant died speedily after removal.

VIII. Coal Gas.

1. A Laburnum was introduced into a jar containing 85 cubic inches, along with 4 cubic inches of coal gas, or 1 in 21¼. In twenty hours its leaves drooped. In twenty-five hours the apex of the main stem had also drooped. The plant, after being left for four days, did not droop further. The cotyledons fell off in the act of removing the plant from under the bell-jar; it however recovered.

2. Into a jar of similar capacity, 50 cubic inches of gas were introduced along with another Laburnum. In twenty-four hours the plant drooped decidedly. It was then removed, and also recovered.

3. A Laburnum and Balsam were placed in a jar containing 180 cubic inches, along with 25 cubic inches of coal gas, or 1 in 7⅕. In twenty hours no perceptible change had taken place. On the fourth day of their exposure to the gas nothing particular was observable. The plants seemed fresh, with the exception of a slight drooping in the stem of the Balsam. Both these plants recovered.

4. Into a jar containing 200 cubic inches, a Laburnum and Balsam were introduced with 4 cubic inches of the gas, or 1 in 50. In twenty hours the cotyledons of the Balsam became slightly curled, while the Laburnum remained unaffected. No further change took place till the fourth day, when the cotyledons of the Balsam were observed to have become much paler and shrivelled, the leaves to have become dry and yellow

at the tips, and to hang down languidly. In the Laburnum, the apices of the leaves had become paler, and fell off when touched in the most gentle manner. Both these plants recovered. These experiments with coal gas seemed to show that, just as we found with sulphuretted hydrogen, when the proportion is large, the effect on the plants appeared to be less than when the proportions were smaller.

To conclude, then, it will be evident from the preceding experiments that gases divide themselves into two classes as regards their action on plants—viz., into narcotic and irritant gases. This distinction, to whatever cause traceable, is as real in the case of plants as in that of animals. When subjected to the influence of a narcotic gas, the colour, it was observed, never became altered, and the plants looked as green and succulent at the end of the experiment as at the beginning. Whenever the plant began to droop, though removed to a forcing-bed, and watered, in no instance did it recover, but died down even more speedily than it would have done if left to the continued action of the gas. In one word, narcotic gases destroy the life of the plant. With irritant gases, on the other hand, the action is more of a local character. The tips of the leaves first begin to be altered in colour, and the discoloration rapidly spreads over the whole leaf, and, if continued long enough, over the whole plant; but if removed before the stem has been attacked by the gas, the plants always recover—with, however, the loss of their leaves. In a short time they put out a new crop, and seem in no way permanently injured; but, of course, if repeatedly subjected to an atmosphere of irritant gas, the plants were destroyed.

On the Capture of Whales by means of Poison. By ROBERT CHRISTISON, M.D., V.P.R.S.E., *Professor of Materia Medica in the University of Edinburgh.*[*]

So long ago as the autumn of 1831 I was requested by a mercantile firm in Leith, at that time engaged in the whale-fishery both in the Arctic and South Seas, to aid them in devising a plan for capturing the Greenland and Spermaceti whales by means of poison. When the project was first put before me by the gentlemen in question, the late Messrs W. and G. Young, I confess that for some time I could not entertain it seriously. But on due explanation the proposal appeared sufficiently reasonable to deserve inquiry; and after inquiry it was resolved to make the trial.

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[*] Read before the Royal Society of Edinburgh, February 6, 1860.

This trial, for reasons which will appear in the following narrative, proved abortive. But, as the Messrs Young were at first not discouraged by that result, it was necessary that all parties concerned should observe silence as to the means and method employed. Hints, nevertheless, were obtained by others; and other trials were accordingly at various times reported in the newspapers; but the reports were so vague that they might have originated merely in rumours of the undertaking of the Messrs Young.

These gentlemen have been dead for some years. They had previously given up the whale-fishery altogether; and it has not been resumed by the representatives of their firm, the sons of one of them. There is no longer, therefore, any occasion for silence on my part. On the contrary, it is right that the originators of the proposal to capture whales by poison should have the credit they deserved, for their ingenuity and enterprise. And, as nothing occurred in their trials to contradict their expectation of success, and late improvements in our knowledge of poisons seem to me to strengthen that expectation, it may prove of service to make the facts now public.

The explanation of their hope of success, given me by Messrs Young, and a very intelligent ship-captain, their whaling-master, was as follows:—

In the Arctic Seas the whale is frequently met with not far from the ice. When struck with the harpoon, the animal commonly makes for the ice; and if it reaches the edge before being overtaken by the boats, it dives under the ice-field, and, coming up again at a distant blowhole in the field, it lives or dies out of reach of its pursuers; who must therefore cut their line, and leave their booty. Occasionally, indeed, the whale may even then be got at and secured. But this is a rare occurrence; and the calculation is that a tenth part of all those struck in a whaling voyage are thus lost under the ice. If the progress of the animal, however, could be either arrested or retarded in any way, so that its pursuers might come up with it in time to attack it with their lances before it reaches the ice, its capture would be generally accomplished. Hence, in using poison to facilitate its capture, although there would be an advantage in killing it, its death is not necessary: to paralyse or enfeeble it would for the most part be enough.

In the South Sea fishery the conditions of the problem are somewhat different, but the solution of it is the same. The spermaceti whale is commonly met with in schools, as the sailors call them. When one is harpooned it runs right ahead, against the wind, and usually to a great distance. Several boats, therefore, may be required to keep up the communication between the boat which is fast and the ship, which cannot beat quickly enough to windward to keep it in view. Hence, to attack more than two of a school at one time might be unsafe; and before these are killed, the rest may escape out of sight. But if the animals could be either promptly killed, or even only paralysed and circumscribed in their movements, more of a school might be secured by their pursuers.

These statements being put before me, I had to consider what poison was fittest for the purpose, by combining a subtile, swift action with the property of impairing voluntary motion, and with the capability of being easily introduced into the body. At that time the only poison known to combine all these conditions was the pure hydrocyanic acid. In rapidity of action it is still unsurpassed. In energy, too, it appeared sufficient. According to Scoresby, the Greenland whale attains a maximum length of sixty feet, and then weighs about seventy tons. But a whaler may be year after year in the Arctic Seas without killing an animal of that size; and I was assured by my authority, the whaling-master of Messrs Young's ship, that practically it would be sufficient to provide for whales of forty feet, weighing towards forty tons. Now a single minim of pure hydrocyanic acid would undoubtedly be adequate to kill, if struck into the flesh, a man of two hundredweight; who, in point of structure in muscle and fat, may be considered to be in his own species the *analogue* of the whale. Hence, if the whale have the same sensibility to the action of hydrocyanic acid as man and land animals generally, two ounces of the poison (875 minims) ought to be enough to stupify at all events, if not to slay, a whale of forty feet, and much more the commoner sort of inferior dimensions. But the blade and shaft of the harpoon are so constructed as to allow of this much being easily sheltered between them in two glass tubes, and introduced, without impediment to the weapon, into any part of the animal's body which the blade may reach. The mode of procedure and object of the harpooner are also favourable. He endeavours to approach quietly close to the whale while at rest on the surface of the water, to strike it under the fin, to penetrate through the blubber into the subjacent flesh, and, if possible, to lodge the blade within the cavity of the chest. At the same time a half turn is given to the harpoon the moment the stroke is completed, in order to obviate the chance of the blade being drawn out by the wound when the animal first starts off. Such was the statement of the whaling-master, himself a dexterous harpooner.

All these circumstances are exactly what the toxicologist would choose as favourable for the introduction of poison into the body of any animal through a wound. But it remained to devise a method of discharging the poison from the glass tubes at the right time. After various trials, the plan fixed upon was to attach firmly to each side of the harpoon, near the blade, one end of a strong copper-wire; the other end of which passed obliquely over the tube, thereby securing it in its place; then through an oblique hole in the shaft, close to the upper end of the tube; and, finally, to a bight in the rope, where it was firmly secured. It is plain that the rope cannot be drawn straight before the copper wire is broken; and the copper wire was so strong, that it could not be broken without first crushing the tubes; to facilitate which, a spiral indentation was made upon the tubes for the wires to lie in. It appears, however, from one of the harpoons which has been recovered through the kindness of the present Messrs Young, that a different and simpler plan had been devised before the vessel sailed for her destination. The blade of the whale harpoon has commonly a double barb thus, fig. 1.* In the poison-harpoons, the ends of the barbs were jointed as in fig. 2.* It is evident, that as soon as the animal sprung off on the harpoon being struck into its body, the ends of the barbs would be pulled open by the drag exerted on the harpoon, and that the inner point of the barbs would be pressed strongly against the glass tubes, and crush them.

Fig. 1.

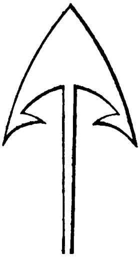
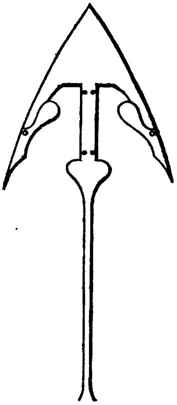


Fig. 2.



The preparation of the poison, at that time not an easy matter, had next to be looked to. One hundred ounces of concentrated hydrocyanic acid were necessary. It was prepared in my laboratory, under my direction, by my assistant, and the gentleman who was to go out as surgeon of the whaling vessel. The method chosen was to distil the acid of the strength of 50 per cent., from ferrocyanide of potassium decomposed by diluted sulphuric acid. As it was imprudent to operate on a large scale with a substance so formidable, several days were necessary; and even thus the scale was large enough to render great caution indispensable. In fact, the manufacture of the poison was not completed without a casualty. The ship's surgeon, an unpractised chemist, was warned by me to keep at a distance from a safety tube, through the fine opening of which hydrocyanic acid vapour was occasionally expelled. But, when familiarity had bred contempt, he could not refrain putting his nose to the tube, to judge how the process was going on,—until at last he suddenly gave a shout, staggered back, and fell insensible in the corner of the apartment. His companion, however, soon revived him.

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* These figures are drawn to measurement, one-tenth the natural size. The blade of fig. 2 measures $4\frac{1}{8}$ inches to the shank; the upper end of the shank, where the poison-tubes are lodged, is 4 inches long, $\frac{8}{10}$ths of an inch wide, and 1 inch thick; the barbs, when closed, as in the figure, are $8\frac{1}{2}$ inches apart at the points.

The acid was put into eight-ounce bottles, as it was made; and the bottles were at once immersed in ice, to prevent decomposition of the acid. In this state it was stowed on board the whaler; and the surgeon was provided with a large quantity of fused chloride of calcium, for removing the water and rendering the acid pure as soon as the vessel should reach the whale regions. This part of our operations would be much simplified nowadays, by the discovery that perfectly pure hydrocyanic acid may be securely preserved without change for a very long time at all ordinary temperatures, in hermetically-sealed tubes. I have a specimen which I obtained from my late colleague, Dr Gregory, which was thus kept entire for eleven years. But it began to change, without discoverable cause, two years ago, and is now a black powder, consisting chiefly of paracyanogen.

In the spring of 1832 the William Young left Leith for the Arctic Seas, provided with fifty harpoons, and the poison required for them. She made a prosperous voyage out. Ere long, it was time for the surgeon to concentrate the acid. He had the mate for his assistant, the captain's cabin for his laboratory, and, for precaution, my advice to work with the deck windows removed, and to rush for the deck the moment any accident should happen. The advice proved not uncalled for. Operations were scarcely begun, when an eight-ounce bottle of poison fell and broke. The mate, obedient to orders, rushed instantly to the cabin stair, and barely reached the deck when he fell down powerless, calling to his companions to "run for the doctor." The captain, who had been fortunately looking on from time to time from the deck, seized a long boat-hook, in a trice hooked up by the waistband the surgeon, as he lay insensible on the cabin-floor, and espying a bucket of salt-water not far off, poured an ample stream over the patient's head. He thus had recourse to the best of all remedies for poisoning with hydrocyanic acid; and his promptitude had its reward in the speedy recovery of the surgeon under his cold douche. As for the chemical assistant, the mate, he was probably as much frightened as hurt, thanks to the speed of his retreat: for he came round very soon without any treatment. The operators were not discouraged by this narrow escape. The acid was duly strengthened, and some tubes and harpoons were charged one evening, when whales were for the first time seen blowing at a distance. But it was too late to pursue them that day. All was made ready in the boats, however, for the earliest dawn next morning.

Here the trial came to an abrupt and untimely end. In the middle of the night the William Young and another whaler got in the way of two fields of ice, in a few minutes their bottoms were crushed to pieces, they became complete wrecks, and the crews had to flee for safety to the ice, whence they were rescued by other friendly vessels.

Within a few weeks, I have learned for the first time, that the Messrs Young, not discouraged by their misfortune, determined to make a second trial. In 1833 they appear to have contemplated a repetition of the attempt with strychnia, as a large quantity of that poison was purchased by them early in the spring. Whether a trial of it was actually made I have been unable to learn. But in that year they fitted out another vessel for the northern whale-fishery, called the Clarendon, with poison-harpoons, and a large supply of hydrocyanic acid. Unfortunately they did not communicate with me on that occasion, so that I have no authentic knowledge of the result. At the period in question, no manufacturing chemist was in the habit of making the strong acid; the preparation of it on a considerable scale was not without risk, and scientific chemists therefore worked with it on a small scale, and prepared it in a way in which it was extremely apt to undergo speedy change and conversion into the inert paracyanogen. I cannot ascertain how the acid was made for the voyage of 1833; but it was obtained in the diluted state, and, as in 1832, it was afterwards to be deprived of water by chloride of calcium. I am indebted to Dr R. Paterson of Leith for having investigated various rumours still existing in that town as to the result of the trial made during the Clarendon's voyage. According to the account of one eyewitness, the poison-tubes were themselves fired from a musket at the whales, and did no harm. The fact is, that a harpoon-gun was provided; and another seaman states that he himself fired it for the first and only time it was used; that the harpoon was buried deeply in the whale, which immediately "sounded," or dived perpendicularly downwards; but that in a very short time the rope relaxed, and the whale rose to the surface quite dead. And he added, that the men were so appalled by the terrific effect of the harpoon, that they declined to use any more of them. The only other certain facts which I have been able to recover, are that twenty-four fish were killed, being the largest number that had been brought to Leith by a single ship for a long period, and that six were lost by the harpoon "drawing," that is, being dragged out of the wound by the strain on the line. The log of the ship, still extant, mentions these facts; but, singular as it may appear, makes no mention of the use of poison at all. The conclusion is, that the Messrs Young naturally desired to conceal their experiment as much as they could; and this circumstance may account for the vague and contradictory accounts, which are still recollected at Leith, of the experiments made on that occasion by the Clarendon.

Rumours of the trial, however, reached other ports. Several notices of similar experiments appeared from time to time, at distant intervals, in the newspapers and popular periodicals; but these notices were too brief and vague to deserve attention. The crew of the Messrs Young's lost vessel would certainly spread the news of the novel harpoons and their purpose, and the trial by the Clarendon would also certainly spread widely. Such intelligence might give rise to other trials. Accordingly, information, too incomplete to bear out any satisfactory conclusion, has reached me, to the effect that a successful trial was made by a whaleship from another port, subsequently to the experiments of Messrs Young. There is a current story in Leith, that a vessel belonging to Aberdeen or Peterhead was provided with harpoons poisoned with prussic acid; that the harpoons were actually used; that the result was completely successful, inasmuch as the whales, when struck, were either killed outright, or were unable to move afterwards, so that they were easily despatched by the men's lances; but that the crew were so alarmed by the terrible action of the poison, that they were afraid to "flense" the whales, dreading the influence of the hydrocyanic acid diffused throughout the bodies of the animals. This is too circumstantial a tale to be altogether destitute of foundation. And besides, it is borne out so far by information communicated to me recently by Dr Andrew Fleming, of the India medical service, and now in Edinburgh, who says that, when a student in Aberdeen in 1838 or 1839, he assisted Dr Shier, then in Aberdeen, but who afterwards settled in Demerara, in preparing concentrated hydrocyanic acid for a trial of its effects in capturing whales, and that he understood the trial had proved satisfactory.

It is clear at any rate, that nothing has yet been ascertained to show that the capture of whales by means of poison is not practicable. On the contrary, whatever is known rather encourages to future trials. And when it is considered that concentrated hydrocyanic acid may now be made with little difficulty to any reasonable amount, and may be preserved in its pure state for several years by very simple means; when it is added that toxicologists now know poisons even more potent than pure hydrocyanic acid—which promptly paralyse, and cause death by paralysis—which may be kept without alteration almost indefinitely—and which, though now rare and costly, may be had cheaply and in abundance so soon as a remunerating demand shall make them an object of attention to the chemical manufacturer,—I confess I shall be surprised if we do not hear of renewed trials and of established success.

On an Oil-Coal found near Pictou, Nova Scotia, and the Comparative Composition of the Minerals often included in the term Coal. By Henry How, Professor of Chemistry and Natural History, King's College, Windsor, Nova Scotia.

The name given to the substance I purpose describing indicates the use to which it is put,—viz., the manufacture of paraffine oil; and an inquiry into the association of elements in the minerals constituting the sources of this and similar "mineral oils" and in the bituminous coals, may possess some interest in a chemical point of view. As regards the classification of these minerals, it is not necessary to do more than recall the attempt made some few years ago in courts of law in Scotland, New Brunswick, and Nova Scotia, to decide what should, and what should not, be called a coal. The great array of evidence of various kinds brought to bear on the question rendered it a very interesting one, and it is well known that the opinions of the numerous scientific men consulted on these occasions were so nearly balanced that the point at issue was determined on the commercial, rather than on the scientific, merits of the cases. It will be remembered that the substances in dispute were the Torbanehill "coal," found near Bathgate in Linlithgowshire, Scotland, and the Albert "coal," occurring at Hillsborough, New Brunswick. As respects the former, the result of the trial in Edinburgh in 1853 was, that the jury considered it to be "coal, in the common sense of that word;" and as regards the latter, it was decided at Fredericton, New Brunswick, and at Halifax, Nova Scotia, in 1851, to be also a "coal." Notwithstanding these legal decisions, which, from the conflicting opinions of witnesses, were obviously arrived at from other than scientific considerations, the question as to what is, and what is not a coal, must be held to be an open one in those sciences in whose province the matter lies, and it will probably long remain so; because it was not from the absence of data, but from differing interpretations of facts about which, for the most part, there was a general accordance, that there arose the well-known want of unanimity among geologists, mineralogists, chemists, and microscopists.

In this paper I do not hope to decide the question; but I wish to point out as interesting facts the occurrence of true bituminous coal in contact with the oil-coal, and to call attention to the relative proportions of the ultimate elements in the latter, and in the before-mentioned disputed substances, as compared with bituminous coals, as important in explaining the different nature of their products of distillation, and in affording support to those who do not make one species only of these minerals.

Some of the analyses which follow are published for the first time; others, of my own, relating to bituminous coals, I have taken from among those given in the Report on Coals suited to the Steam Navy of Great Britain, by Sir H. Delabeche and Dr Playfair; and those of cannel coals are taken from sources hereafter indicated.

The oil-coal found near Pictou, Nova Scotia, was first met with by persons residing in the neighbourhood early in 1859, and its exact locality is called Fraser Mine. It occurs in the Coal Measures. I am indebted to Henry Poole, Esq., manager of the Fraser Mine, for the following particulars relating to the geological position, &c., of the substance:—

"The lowest measures, about sixty yards on the surface, short of where the oil-coal crops, are chiefly composed of strong bands of sandstone,—actual thickness not yet proved; thin shales with bands of ironstone, and stigmaria roots, with sigillaria stems, and a few detached fern-leaves in such soft shale that I have not been able to preserve any good specimens. Immediately above the oil-coal is a seam of bituminous coal about fourteen inches thick. Where we commenced to open a mine by driving a slope, the oil-coal was fourteen inches thick, but at two hundred feet down at the bottom of the slope the oil-coal was twenty inches thick; it has a smooth regular parting at top next the coal, as also at the bottom next to the oil-batt below, but throughout its entire thickness it is of a curly twisted structure, many of its fractures look like the casts of shells, and the sharp edges are polished of a slickensides character. No fossils that I am aware of have hitherto been found in the curly oil-coal. The oil-batt next below is nearly two feet thick; of a homogeneous character, with a slaty cleavage of various thicknesses. In this band two or three varieties (species?) of Lepidodendron, beautifully preserved, have been found, also Poacites or leaves about ¼ inch wide, and in lengths of from four to six inches, which have undergone so little change, that, when the damp shale was fresh split, they could be removed, and were so elastic that they could be bent considerably without breaking. At the bottom of the slope, another thin seam of curly oil-coal has appeared of a few inches in thickness, but is not worked at present. In the roof-coal were found pieces of decayed wood very little changed, which I consider a great curiosity. On M'Lellan's Brook shale is above the oil-coal, and oil-batt below, in which have been found Lepidodendra, and apparently molar teeth with three fangs, flattened modiola shells, and spines or small fish teeth. The oil-batt has been found in several places without the curly band, or so-called oil-coal. Two thousand tons of oil-coal have been raised (Dec. 1859) at the Fraser Mine."

The oil-coal varies in colour from brown to black, is dull where not polished, as just mentioned, has a reddish-brown lustreless streak, its powder is dark chocolate coloured, it is very tough, and breaks at last with a hackly fracture; its specific gravity in mass, after the vessel of water containing it had been in an exhausted receiver=1·103. It takes fire very readily, and when removed from the lamp still burns for some time with a brilliant smoky flame, and flaming melted fragments continually drop from it in a truly characteristic manner. Ignited in coarse powder in an open crucible, it gives off abundant smoke and flame; then seems to boil quickly, and a "coke" is left of the bulk of the original material, and showing, when turned out, a perfect cast of the interior of the vessel. The ash of the "coke" is gray, and consists mainly of silicate of alumina; at least no lime, or a mere trace, is dissolved by acid, while some alumina is taken up, and a good deal of solid remains undissolved. The powdered oil-coal, digested with benzine and with ether, does not more than sensibly colour these liquids, but some residue remains on evaporation in each case. The bituminous coal occurring with the oil-coal had the usual characters of its species—it was black, brilliant, and very brittle. The proximate analysis of the two are placed side by side—

	Oil Coal.	Bitum. Coal.
Volatile Matters, . .	66·56	33·58
Fixed Carbon, . .	25·23	62·09
Ash, ...	8·21	4·33
	100·00	100·00

and it is obvious that they contrast very strikingly. The following is the ultimate analysis of the oil-coal, for which I am indebted to Mr Slessor, assistant to Professor Anderson of Glasgow, whose aid I requested, from want of the necessary apparatus:—

Carbon, ...	80·96
Hydrogen, ...	10·15
Nitrogen,[*]	0·68
Ash (above), ...	8·21
	100·00

The oil-batt appears to be decidedly a shale; a specimen from Bear Brook, Fraser Mine, gave these results:—

Volatile Matters, ...	30·65
Fixed Carbon, ...	10·88
Ash, ...	58·47
	100·00

I proceed to compare the Torbanehill mineral and the Albert "coal." A specimen of the former, examined at the time of the trial before mentioned in 1853, gave me—

Volatile Matters,	71·17	Carbon, . . .	66·00
Fixed Carbon, .	7·56	Hydrogen, . .	8·58
Ash, ...	21·18	Nitrogen, . . .	0·55
		Sulphur, . . .	0·70
	100·00	Oxygen, . . .	2·99
		Ash, ...	21·18
			100·00

and in a recent examination, a specimen of Albert "coal" gave—

Volatile Matters,	54·39	Carbon,[†] . . .	87·25
Fixed Carbon, .	45·44	Hydrogen, . .	9·62
Ash, ...	0·17	Nitrogen, . . .	1·75
		Oxygen and S, .	1·21
	100·00	Ash, ...	0·17
			100·00

These results I place in a table with corresponding data, obtained from bituminous and cannel coal, the specific gravities

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[*] With O and S.

[†] This analysis as regards C, H, and N, was kindly furnished by Mr Slessor.

Kind of Substance.	Name or Locality.	Spec. Gravity.	Proxim. Anal.		Ash.	Ultimate Analysis.					Ratio of Carbon to Hydrogen.	Observers.
			Volatile Matters.	Fixed Carbon.		Carbon.	Hydrogen.	Nitrogen.	Sulphur.	Oxygen.		
Welsh Bituminous Coals, . . .	Powell's Duffryn .	1·326	15·70	81·04	3·26	88·26	4·66	1·45	1·77	0·60	100 : 4·82	H. How.
	Mynydd Newydd .	1·310	25·20	71·56	3·24	84·72	5·76	1·56	1·21	3·52	100 : 6·79	H. How.
	Ebbw Vale . . .	1·275	22·50	76·00	1·50	89·78	5·15	2·16	1·02	0·39	100 : 5·73	H. How.
Scotch Bituminous Coals, . . .	Grangemouth, . .	1·290	43·40	53·08	3·52	79·85	5·28	1·35	1·42	8·58	100 : 6·61	H. How.
	Fordel Splint, . .	1·025	47·97	48·03	4·00	79·58	5·50	1·13	1·46	8·33	100 : 6·93	H. How.
English Bituminous Coals, .	Broomhill, . . .	1·025	40·80	56·13	3·07	81·70	6·17	1·84	2·85	4·37	100 : 7·55	H. How.
	Parkend, Sydney, .	1·283	42·20	47·80	10·00	73·52	5·69	2·04	2·27	6·48	100 : 7·73	H. How.
English Cannel, .	Wigan, ...	1·276	39·64	57·66	2·70	80·07	5·53	2·12	1·50	8·08	100 : 6·90	Vaux.
Scotch Cannel Coals, . . .	Lesmahagow, . .	1·251	56·70	37·26	6·034	73·44	7·62	?	1·145	? *	100 : 10·43	W. A. Miller.
	Capledrae, . . .	?	?	?	25·40	56·70	6·80	1·90	0·35	8·80	100 : 11·99	A. Fyfe.
Scotch . . . . ?	Torbanehill, . .	1·170	71·17	7·65	21·18	66·00	8·58	0·55	0·70	2·99	100 : 13·00	H. How.
?	Hillsborough, N.B.,	1·091	54·39	45·44	0·17	87·25	9·62	1·75	?	? †	100 : 11·02	H. How & J. Slessor.
?	Pictou, N.S., .	1·1039	66·53	25·23	8·21	80·96	10·15	0·68 ?	?	‡	100 : 12·53	How and Slessor.

* N and O = 11·761 per cent. † S ? and O = 1·21. ‡ N, S and O = 0·68.

of the substance, the ratio of carbon to hydrogen as calculated directly from the analysis, and the authority for the numbers. The first seven analyses are from the "Report on Coals" by Sir H. Delabeche and Dr Playfair, 1848, and given also in "Memoirs Geol. Survey," vol. ii.; the eighth and ninth from "Miller's Chemistry," iii. p. 201; the tenth from "Report of Trial on Torbanehill Coal," Edinburgh, 1853; the eleventh has not been yet published in any book.

In this table we observe, in the first place, the resemblance of the last three substances, in having a density much below that of all the others; and secondly, that in all the bituminous coals but one, the volatile matters are considerably less in amount than the fixed carbon, while in the cannel coals this is also the case with one of the two whose proximate analyses are given; as regards the other, we see that it contains a large percentage (=11·761) of O and N, which would, of course, be included as volatile matters; and in the last three substances the volatile matters greatly exceed the fixed carbon. It is well known that in discussions on the chemical nature of coals, &c., much stress is laid on the relative proportions of these products, and also on the ratio of carbon to hydrogen; but it appears to me that an important element in the calculation has generally been omitted, or has not received due attention—I allude to the quantity of oxygen present, which of course can only be found by ultimate analysis. It is constantly stated that the gas and oil producing value of a coal is indicated by the weight lost in coking; but this is obviously true only to a certain extent, and indeed is in some cases clearly untrue; for if we do not take into account the effect of oxygen present, we cannot make a just comparison of the chemical nature of the substances, nor find the ratio of C : H, neither can we give the real gas or oil value, when, as above, from 8 to 10 per cent. of what is generally supposed to be carbon and hydrogen is really oxygen and nitrogen. If, for example, we consider the effect of the oxygen in the composition of the substances given in the table, we shall see that the last three present such differences from the others as to strengthen the position of those who decline calling them "coals." Limiting our view to the cannel coals, which, as seen above, exhibit the ratio of $C : H$ apparently equal, or nearly so, to that in the substances in question, we observe that they all contain much more oxygen, and if we deduct the equivalent quantity of hydrogen in all, as is theoretically necessary for arriving at the heating power, we shall find this similarity greatly lessened; as thus:—

Ratio of $C = H$ *after deducting* $O = H$.

Cannel Coal	from	Wigan,	100 : 5·65
„	„	Lesmahagow,	100 : 8·71[*]
„	„	Capledrae,	100 : 10·05
?	„	Torbanehill,	100 : 12·43
?	„	Hillsborough,	100 : 10·85
?	„	Fraser Mine,	100 : 12·43

And the last three substances should prove, theoretically, the excellent "oil coals" they are known to be. Of course the practical yield of oil will vary according to the manipulation, the perfection of the manufacturing processes, and the quality of samples employed; but the following statement of the comparative amounts of oil afforded by some of the above may be taken as a good illustration of the point brought forward in this paper. I am indebted for these details to H. Poole, Esq.:—

In Scotland, the Lesmahagow cannel coal gives 40 gallons crude oil, and 32 gallons rectified oil, per ton.

At M'Lellan's Brook, the Fraser oil-coal gives 40 gallons crude oil per ton.

At Coal Brook, the Fraser oil-coal and oil-batt, together, give 53 gallons per ton.

At M'Culloch's Brook, the Fraser oil-coal gives 77 gallons per ton.

The "Albert coal" gives 100 gallons per ton.

The Torbanehill "coal" gives 125 gallons per ton.

And some picked samples of oil-coal from Fraser Mine, tried in Boston, U.S., gave no less than 199 gallons of oil per ton.

Abstract of Experiments with Anæsthetic Agents on Sensitive Plants. By Mr William Coldstream.[†]

In this paper I have given an abstract of some experiments performed in the summer of 1859 on various sensitive plants, with the view of ascertaining the effects of anæsthetic agents on vegetable irritability. The subject was suggested by Professor Balfour, who offered a prize for it in his class.*

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[*] After allowing 2 per cent. for nitrogen.

[†] Read to the Botanical Society, June 14, 1860.

The experiments were conducted under most favourable circumstances, as far as the state of the plants was concerned. When a warm temperature was required, as in the case of the *Mimosa sensitiva* and *M. pudica*, accommodation was provided in the hot-houses at the Royal Botanic Garden. The agents employed were chloroform, sulphuric ether, amylene, and chloric ether.

The chloroform was used both pure and dilute. In the narration of the experiments, where not otherwise specified, pure chloroform is to be understood. Where the vapour of these substances was required, the plan taken was as follows:—

The bell-glass under which the plant or flower was to be placed was inverted and the necessary quantity dropped in; then, being quickly reversed, it was placed over the subject of the experiment.

In other cases, a piece of blotting-paper moistened with the required quantity was pushed under the edge of the glass, raised for an instant to receive it. Communication with the external air was prevented by the glass being placed on moist leather; or, when it stood on a wooden board, as was the more usual way, a rim of putty was put round its edge.

Experiments were first tried to discover the effect produced by the *actual contact* of the anæsthetizing agent with the sensitive leaves of the *Mimosa pudica*. Professor Marcet's observations on the subject, in vol. xlvi. of the Edinburgh New Philosophical Journal, were taken as a basis for these, but the results obtained were not satisfactory—no true anæsthesia being produced.

We now proceed to give the results of experiments made with the vapour of chloroform and amylene inhaled by the plant. These varied very much, according as the quantity of the agent mixed with the air surrounding the plant was great or small. Every result, varying between absence of all effect and speedy death, was obtained, as will be seen by the experiments to be detailed.

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* Mr Coldstream's Essay was rewarded with the Prize then offered.—Ed. *New Philosophical Journal.*

Anæsthesia has been manifestly induced in a very limited number of instances; a very common effect when the vapour was weak was, a total or partial closure of the folioles when first exposed to it, followed in a short time by complete expansion, as if they became accustomed to the action of the chloroform. This is paralleled by an instance recorded by Desfontaines, who relates that, as he carried a sensitive plant one day in a carriage, the jolting of the vehicle caused at first contraction of the folioles, which after some time expanded, as if habituated to the movement.

In cases in which this re-expansion, while still exposed to the vapour, took place, diminution in sensibility was never observed on removing the glass. The vapour was then apparently (though sufficient to act as an irritant) not sufficient to produce an anæsthetic effect. But even when strong enough to cause the continued contraction of the folioles, either completely or partially, insensibility was far from an invariable effect. When, however, it did occur, it was usually in those cases where the folioles remained in a half contracted state.

Exp. 13. A plant was exposed in a jar of 185 cubic inches capacity to the vapour of five minims of chloroform. In two minutes the leaflets began to close slowly. The closure was only partial. In five minutes the glass was removed, the leaflets continuing half closed; when irritation was applied, the sensibility was found somewhat diminished. In this experiment, the proportion of chloroform was one grain to thirty-seven cubic inches of air.

Exp. 14. A plant was exposed in a vessel of 185 cubic inches capacity to the vapour of two minims of chloroform, being a minim to 92½ cubic inches. In ten minutes the folioles were all partially closed. In half an hour many still continued so, while others opened; and on removing the glass jar, the sensibility of those which were open was apparently slightly diminished. The plant soon recovered.

Exp. 15. A plant was exposed in a vessel of 185 cubic inches to the vapour of two minims of chloroform. In two minutes some of the folioles began to close, some reopened, others continued half-shut. In fifteen minutes the glass was removed, and the sensibility was found slightly diminished. The plant soon recovered.

Exp. 17. A plant was exposed in a vessel of 345 cubic inches capacity to the vapour of two minims of chloroform, mixed with methylated spirit, in the proportion of one to two. In four hours and ten minutes, slight anæsthesia was observed in the youngest leaves. The plant regained its sensibility.

Exp. 18. A plant was exposed in a vessel of 345 cubic inches capacity to the vapour of two minims of chloroform. In five hours the plant was removed, the leaves being then closed. On being examined the next day, the leaflets were found expanded, and the plant apparently healthy; but on applying irritation, it was seen that the sensibility of most of them was quite gone. On the third day they recovered.

Exp. 19. A plant was exposed in a vessel of 345 cubic inches capacity to the vapour of one minim. When removed in sixteen hours, the leaflets were found all open, and the plant apparently healthy, except that the leaf-stalks were slightly depressed. When irritation was applied, however, it was seen that they were quite insensible. The next day, the anæsthetic effect was still marked. On the third day they had recovered much of their irritability.

Exp. 23. A plant was exposed in a vessel of 185 cubic inches to the vapour of five minims of amylene. It was removed in one hour, when the tender upper leaflets were half-closed, the lower ones expanded. The upper leaflets were quite insensible to touch; the excitability of the lower ones was also greatly diminished. The leaflets were long in recovering. In half an hour the half-closed leaflets were still insensible to touch, and the lower ones very partially so. The complete recovery of this plant is uncertain.

Exp. 24. A plant was exposed in a vessel of 345 cubic inches capacity to the vapour of 17 minims of amylene. In ten minutes the pinnæ had begun to close with a jerking motion. In fifteen minutes all the upper leaflets were tightly closed, the lower ones being still expanded. These latter showed no diminution of sensibility when the plant was now removed. In a short time the upper ones began to open, and when half expanded, it was seen that their contractibility was lost, and that they had assumed a somewhat shrivelled appearance.

Exp. 25. A plant was exposed in a vessel of 344 cubic inches capacity to the vapour of two and a half minims, being nearly one minim to 135 cubic inches. It was allowed to remain fifteen hours and a half. Being removed at the end of that time, it was found almost completely insensible to touch. The upper leaflets seemed least affected, and soon regained a large measure of sensibility, but the lower ones continued perfectly insensible, while, at the same time, fully expanded, and to all appearance healthy. This state continued for three days after, when the folioles of the lower leaves began to drop off.

The results obtained by exposing the irritable stamens of the *barberry* to the action of chloroform, amylene, &c., have furnished by far the most satisfactory proof of a true anæsthetic condition in plants. Two species were experimented on —the British *Berberis vulgaris* and an American one. The character of the results was most interesting, and their uniformity remarkable. Immediately after exposure to the vapour, the irritative action, as in man and animals, first set in; that is to say, the irritable stamens of the flower sprung towards the pistil. This action was instantaneous; but almost immediately they began to move slowly back to their former position, till in a few minutes they were seen to be again appressed to the petals. If now removed from the bell-glass, the stamens were found to be *destitute of irritability.* Irritability was never lost until the stamens had thus sprung; and in the case of flowers, some of whose stamens only were thus irritated, it was found that those which had *not* sprung showed undiminished sensibility, while the others had lost every trace of it. Here was true anæsthesia; for if the flowers were now taken and exposed to the warm sun, they were, with very few exceptions, restored to their original irritable condition. They were exposed in bunches of from two to six, so that by one exposure many experiments were, in reality, tried. One bunch of strong young flowers of the *Berberis vulgaris* was exposed four successive times to the action of chloroform vapour, losing its sensibility in each exposure, and then recovering it in the sunshine. In the experiments with barberry the exposures were usually short, and the vapour employed comparatively strong.

I. *Chloroform.*

Exp. 27. Flowers were exposed to the vapour of one minim in a vessel of 13 cubic inches capacity. All irritability was lost in the course of three minutes. It was completely restored twenty minutes after being removed from the vessel.

II. *Amylene.*

Exp. 30. Flowers were exposed in the same vessel to the vapour of three minims, being one minim to 3½ inches, nearly. In ten minutes there was decided diminution of irritability. It was soon restored.

Exp. 32. Flowers were exposed in a vessel of 6·69 cubic inches capacity to the vapour of one minim. In twenty minutes insensibility was produced. They recovered within ten minutes.

Exp. 33. Flowers were exposed in the same vessel to the vapour of two minims, being one mimim to 3·34 inches. In seven minutes the sensibility was destroyed. It was restored in fifteen minutes.

III. *Sulphuric Æther.*

Exp. 34. Flowers were exposed in a vessel 19 cubic inches capacity to the vapour of five minims of sulphuric æther, being one minim to 4 inches nearly. Not affected in five minutes.

Exp. 35. Flowers were exposed in the same vessel to the vapour of five minims. In half an hour their sensibility was completely deadened. They recovered in twenty minutes.

Exp. 36. Flowers were exposed in the same jar with ten minims, being nearly one minim to 2 cubic inches. In half an hour completely deadened. They recovered only partially.

IV. *Chloric Æther.*

Exp. 39. Flowers were exposed in a vessel of 11·5 cubic inches capacity to the vapour of one minim chloric æther. They were not affected in twenty minutes.

Exp. 40. Flowers were exposed in the same vessel to the vapour of ten minims. In half an hour their irritability was gone. They partially recovered.

A *resumé* of the experiments made with barberry is given in the tables attached. Numerous other experiments of the most satisfactory character might have been quoted, but it is believed that what have been given are good average specimens, and that any accession to their number would only have confirmed the facts which they illustrate.

Similar experiments were made on the irritable stamens of the *Helianthema* and the column of the *Stylidium*, but no true anæsthesia was marked in the case of either. The *irritating* effect of the agents was abundantly manifest, but this was succeeded either by the speedy death of the flower, or its recovery in a short time, without having passed through any state which could be safely considered as really one of anæsthesia.

TABLES OF EXPERIMENTS WITH BARBERRY.

I. *Chloroform.*

Minims per 10 cub. in. of air.	Length of exposure. h. m.	How affected.	Result.
·37	0 2	Not at all	...
·26	0 6	Sensibility diminished	Recovered in 20 minutes
·52	0 4	Irritability almost gone	Do.
·76	0 3	All sensibility lost	Do.
·86	0 3	Do.	Do.

II. *Amylene.*

Minims per 10 cub. in. of air.	Length of exposure. h. m.	How affected.	Result.
1·9	0 2	Not at all	...
2·8	0 10	Sensibility diminished	Recovered
1·5	0 20	Total insensibility	Recovered in 10 minutes
2·9	0 7	Do.	Recovered in 15 minutes
6·6	0 18	Do.	Did not recover

III. *Sulphuric Æther.*

Minims per 10 cub. in. of air.	Length of exposure.	How affected.	Result.
	h. m.		
2·6	0 5	Not at all	...
2·6	0 30	Completely anæsthetised	Recovered in 20 minutes
5·2	0 30	Do.	Partially recovered
4·7	0 20	Do.	Recovered in 10 minutes
4·7	0 7	Do.	Recovered in 10 minutes

IV. *Showing the Comparative Action of the different Agents.*

Agent.	Minims per 10 cub. in of air.	Length of exposure.	How affected.	Results.
		h. m.		
Chloroform	0·37	0 2	Not at all	...
	0·76	0 3	Sensibility lost	Recovered in 20 minutes
Amylene	1· 9	0 2	Not at all	...
	2· 9	0 7	Sensibility lost	Recovered in 10 minutes
Sulphuric Æther	2· 6	0 5	Not at all	...
	4· 7	0 7	Sensibility lost	Recovered in 10 minutes
Chloric Æther	0·95	0 20	Not at all	...
	9· 5	0 30	Sensibility lost	Partially recovered

On the Physical Relations of the Reptiliferous Sandstone of Elgin. By the Rev. W. S. Symonds, F.G.S.

I have frequently been asked to give my opinion on the physical geology of the celebrated district of Elgin, North Britain, and to express my candid belief respecting the geological position of the far-famed sandstones which have furnished the relics of those remarkable reptiles the *Telerpeton, Stagonolepis,* and *Hyperodapedon Gordoni.* I have drawn up this short paper, comprehending my observations and notes, with great diffidence; for those observations have induced me to entertain a different opinion, respecting the age of the reptiliferous sandstones, to that expressed by such truly eminent geologists as Sir R. Murchison and Professor Ramsay, at the late meeting of the British Association at Aberdeen; also to the decision arrived at by Mr Patrick Duff of Elgin, Professor Harkness, the Rev. Mr Gordon of Birnie, and Mr Martin of Elgin, all well known authorities.

The geology of Moray has been described at considerable length by Sir Roderick Murchison in the "Proceedings of the Geological Society;"* by Mr Patrick Duff in his "Sketch of the Geology of Moray;" by Mr Martin of Anderson's Institution, Elgin;† and by Mr Gordon of Birnie.‡ I shall not, therefore, attempt any description of the district in detail, but confine my remarks to *those particular points,* in the physical geology, wherein I differ from Sir Roderick Murchison, whose opinions are accepted by the gentlemen I have already named.

I take the *sections* given by Sir Roderick in the "Proceedings of the Geological Society" as illustrative of the usual acceptation of the geology of Elgin, and as I can thus best indicate the *points* which I think require revision, and are capable of a different interpretation. We will first take into consideration Sir Roderick's section from the crystalline rocks of Manoch Hill across the old red sandstone to the yellow sandstone of Elgin.§

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* Proc. Geol. Soc., Aug. 1859.

† Essay on the Geology of Morayshire.

‡ Edinburgh New Philosophical Journal, January 1859.

§ See Quart. Jour. Geol. Soc., Aug. 1859, p. 424.

Cornstones.—I visited this section* in company with my friend Professor Harkness, and under the guidance of the Rev. Mr Gordon of Birnie, after having made the acquaintance of the siliceous, marly, miscalled *Cornstone*, of Linksfield, Spynie, Inverugie, and Lossiemouth.

In so limited a district as that of Elgin, geologists will allow that, without there is some great and apparent metamorphism by intrusive trap, the *lithological and mineralogical features* of rocks are tolerably persistent and good guides in working out the physical position of beds.

Now the rock of siliceous marl at Glass Green, Linksfield, Spynie, and Lossiemouth, called "cornstone," is so *entirely unlike* any other cornstone in the Elgin and Findhorn districts, that I do not understand how any geologist could correlate them as the same. I agree with Sir Roderick Murchison as to the necessity of separating this siliceous "cornstone" of Spynie and Lossiemouth from the cornstone of Foths and that of Cothall on the Findhorn. It is indeed a most "*distinguishable*" *rock.*

I passed twice over the celebrated Findhorn section to Cothall, the first time with Professor Harkness, and the second in company with Sir Charles Lyell and Mr Gordon of Birnie, after having studied the rocks in the Elgin district. This section is well described by Sir Roderick Murchison.†

Whether the mottled cornstone, *d*, of *Foths* (see Sir Roderick's section), may be considered as the equivalent of the cornstones of the Findhorn at Cothall, I consider doubtful. Sir Roderick, I imagine, believes these beds to be *distinct.* They belong to the epoch of the Old Red Sandstone, as proved by their organic remains; and whether they are one band of rock on the same strike, or distinct divisionary cornstones of the Old Red rocks, it matters very little, so long as they are not confounded with the siliceous marly rock of Glass Green, Linksfield, Spynie, and Lossiemouth, to which I now invite attention. This siliceous rock at Glass Green, described by Sir Roderick as cornstone much thicker than the cornstone (of Foths) *d* (section), is rightly represented as dipping *towards* the yellow sandstone of Quarry Wood Ridge, at Elgin. But does it dip *under* that sandstone? I think not. I believe that this is the equivalent rock of that of Linksfield, Spynie, and Lossiemouth, which Sir Roderick has treated as a *higher* band.

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* Proc. Geol. Soc., Aug. 1859, page 424.

† Pp. 422 and 423 of paper in Geol. Journal.

Sir William Jardine will, I am sure, remember that, on the first occasion of our visit to the *Linksfield* section, after we had obtained information upon our points and bearings, I exclaimed, "Why, the dip of this cornstone is *away* from that Quarry Wood Ridge, and not *under* it!" And I very much doubt that a single instance can be shown of the siliceous cornstone of Linksfield dipping in any other direction than *away from* the Quarry Wood Ridge, north of the river Lossie, which river I hold to occupy a line of fault. Sir Charles Lyell and Professor Harkness will both bear witness that the siliceous "cornstone" of Linksfield may be seen on the river banks, west of Elgin, dipping *into* the river, *away* from the Quarry Wood Ridge, and in a directly opposite direction to the siliceous cornstone of Glass Green, on the opposite side the river. My idea is, that *everywhere*, save Glass Green, and of course that line of strike, the siliceous cornstone dips away from the yellow sandstone of Quarry Wood Ridge, and therefore does not *underlie* that sandstone. This is a point of great importance in determining the physical geology of this most difficult district; for I do not think it possible to separate the Glass Green cornstone from that of Linksfield, Spynie, and Lossiemouth. Mr Duff, in his "Sketch of the Geology of Moray," mentions that great difference of opinion exists as to the exact position of the siliceous cornstone of Linksfield, some persons supposing that it passes under the *reptiliferous* sandstones of Lossiemouth, &c.; but Mr Duff maintains, and I think truly, that "no instance can be pointed out in Morayshire of its passing *under* the sandstone, while it certainly overlies and passes into it." Sir William Jardine pointed out the spot, where Sir Roderick Murchison gives a section, showing the siliceous cornstone on the sea-shore, *overlying* the whitish reptiliferous sandstone, with the Stagonolepis, at Lossiemouth. In short, I think it is impossible to doubt that the siliceous cornstone of Lossiemouth, Spynie, and Elgin, *overlies* the reptiliferous sandstones; and it was owing to this evidently overlying position, I do not doubt, that Sir Roderick Murchison separated the Glass Green siliceous rock—which, owing to a reversal of dip, *appears* to dip under the yellow sandstone of Quarry Wood Ridge—from the Lossiemouth and Spynie rock, and induced him to consider the Lossiemouth and Spynie rock as a higher band of cornstone than that of Glass Green.

The point, therefore, to which I think it necessary to call attention here, is not the position of Sir Roderick's cornstone (*f* of his section), but the fact that the dip towards the Quarry Wood Ridge is not the true dip, and that the Glass Green cornstone is the same as the Linksfield cornstone, the river cornstone, and dips in reality *away* from the Quarry Wood Ridge, just as the Keuper sandstones of the Vale of Worcester dip away from the upheaved Malverns, wherever the beds are not broken off by a fault and the dip reversed.

Supposing that the siliceous cornstones of Glass Green, Lossiemouth, Spynie, and Linksfield *overlie* the reptiliferous sandstone, what are these sandstones, and what relation do they bear to the yellow Holoptychian sandstones of Quarry Wood Ridge?

Reptiliferous Sandstone and Holoptychian Sandstones.—It is no doubt a very difficult problem to determine whether the reptiliferous sandstones of Spynie, Findrassie, and Lossiemouth really pass into the yellow sandstone, containing *Holoptychii*, at Bishop Mill, just north of Elgin, and at the Hospital quarries further to the west.

It appears to me that one or two points require particular attention.

My friend Professor Harkness detected, at the entrance to the Bishop Mill quarries, on the Elgin side, a mass of greyish sandstones dipping *away* from the Holoptychian ridge towards Elgin and Linksfield, in a position that would bring them under the Linksfield cornstone, and which looks marvellously like not a fault on the strike of the Holoptychian sandstones but the *reptiliferous sandstones faulted against* the Holoptychian ridge, and dipping *away* from it, just as the siliceous "cornstone" is seen to do in two other places, as already indicated, on the south or Elgin side of Quarry Wood Ridge.

It is also my belief that indications of the reptiliferous sandstones resting against and dipping away from the Holoptychian sandstones of the Hospital quarries, may be observed at the south or Elgin entrance to those quarries, not far from the spot where the siliceous "cornstone" is seen to dip towards the south from the Quarry Wood Ridge. Indeed, if they quarry at the base of the Hospital quarries, or the Bishop Mill quarries, on the south side of the Quarry Wood Ridge, I fully expect to hear that the relics of *Stagonolepis* have been discovered in close proximity with the yellow sandstones containing the remains of *Holoptychii,* just as at Findrassie they have been detected near the line of junction. If, however, it can be determined, and I think it will, that the reptiliferous sandstones *dip away on all sides* from the Holoptychian sandstones, there can be no conformable upward continuation of the Old Red rocks into the reptiliferous sandstones. There is a *physical break.*

In a paper I received only a few days since on the geology of Moray, by Mr Patrick Duff of Elgin, and which was read lately at a meeting of the Elgin Literary and Scientific Association, Mr Duff describes a chocolate-coloured rock at Stotfield, which underlies the Findrassie reptiliferous "rose-coloured" beds, and hints that it is possible that this bed may turn out to be the line of separation between the Old Red Sandstone and the Trias. I fully believe that it is on this horizon that the break occurs.

Linksfield Section.—We would now direct attention to the Linksfield section.

We were informed, at the Meeting of the British Association at Aberdeen, that the low hill of Linksfield, which is made up of strata which overlie the siliceous cornstone (a mass of boulder-clay being intercalated), is an oolitic Wealden *patch, not in situ.* At Elgin we heard that it is an oolitic boulder borne from Cromarty or Sutherlandshire by an iceberg, and dropped upon the till which overlies the siliceous cornstone.

The bedding of the Linksfield strata, and the position of the fossils imbedded in the separate beds, were carefully worked out by Mr Charles Moore of Bath, who is an excellent authority on the fossils of the Lower Lias and Trias; and by him the so-called "Oolitic Wealden" strata and fossils were shown, most probably, to be neither more nor less than a section of those remarkable *transition* beds between the *Lias* and *Trias* which are well known to the geologists of the west of England. This Lias and Trias has been worked out along an escarpment for the distance of from 200 to 300 yards; and it is difficult to imagine that any ice-raft, however accommodating, could have carried, wholesale and in detail, so perfect a specimen of an ancient cliff, without deranging the bedding of the strata or disturbing the repose of the fossils.

With regard to the Lias fossils having been transported from Cromarty, I afterwards visited the Eathie beds, so vividly described by Hugh Miller, and obtained the loan of some typical specimens both from Eathie and Shandwick, from Miss Catherine Allardyce and Lieut. Patterson of Cromarty. These specimens have been carefully examined by some of our best Gloucestershire palæontologists, and are pronounced to belong to the epoch of the *Upper Oolite*, instead of that of the *Lower Lias*. There is doubtless a considerable difficulty respecting the till which underlies this Lias and Trias escarpment; but good answers to that difficulty have been propounded by Sir Charles Lyell and Captain Brickenden. Sir C. Lyell supposes that a range of cliffs, of Triassic and Lower Liassic beds, rose above the vale of Elgin during the glacial epoch, when ice-rafts and drifting bergs, with all the phenomena of an Arctic sea, swept down that vale, then a frith; and that the siliceous cornstone was then the actual sea-bed. The icebergs and drifting masses undermined the soft marls of the Upper Trias and Lias, and in time produced a landslip. The whole side of a sea-cliff slipped down from its position, on to a beach of boulder-clay, without any bouleversement of the strata. Such phenomena, we know, have occurred both in Denmark and in the Isle of Wight. The slip then appears to have soon been covered by a thick deposit of boulder-clay and drift, which preserved the strata from further denudation.

Captain Brickenden supposes that the soft beds of the Upper Trias may have been grooved, and that ice-action intercalated the boulder-clay into the grooves and interstices between the hard cornstone and the soft beds above. Either of these propositions seem to me more probable than the wholesale wafting of a cliff from some distant locality. I cannot think that the Linksfield Trias and Lias can be far from their original site of deposition.

If it be admitted that we have powerful evidence of Lower Liassic and Triassic rocks at Linksfield *in situ*, or nearly *in situ;* if the Lias and Trias overlie the siliceous cornstone, not only at Linksfield but in one or two other localities, one out at sea beyond Lossiemouth; and if the siliceous cornstone overlies the reptiliferous sandstones, and all these rocks *dip away* from the Holoptychian sandstones of Quarry Wood Ridge, I think there is a strong case to be made out by the physical geologist in support of the Triassic age of the reptiliferous sandstones.

When visiting, also, the coast-ridge section from Lossiemouth to Burgh-Head, and beholding the pebble-beds of Burgh-Head and the ripple-marked and reptile-trodden sandstones of Clashan, it is impossible for any geologist acquainted with the New Red of England not to feel a strong bias in favour of the Triassic age of the Elgin deposits under notice. I have no wish to intrude my opinion, but when asked I must honestly give it; and I must declare that I never (although I tried very hard to do so) could understand what possible reason, judging from lithological and mineralogical characters (with the exception of the Holoptychian sandstones of Quarry Wood Ridge), there can be for correlating the rocks of the Findhorn with those of Elgin. The conglomerate beds of Cothall do not appear to me to be more like the pebble-beds of Burgh-Head than the millstone-grit of South Wales is like the pebble-beds of the Trias; and the cornstone of Cothall is very dissimilar to the siliceous rock of Linksfield. The general dip of the beds, and the position of the rocks, no doubt seem to demonstrate that all these strata constitute one united mineral series; but I think this difficulty may be overcome, and will be overcome, when the geological surveyors come to work out this difficult district *in parvis et extremis.*

REVIEWS AND NOTICES OF BOOKS.

Mind and Brain; or, the Correlations of Consciousness and Organisation, with their Applications to Philosophy, Zoology, Physiology, Mental Pathology, and the Practice of Medicine. By Thomas Laycock, M.D., F.R.S.E., &c. With Illustrations. 2 Vols. Sutherland & Knox, Edinburgh. 1860.

This is a work of no ordinary comprehensiveness. Viewed in its title and contents, it is indeed a complete anthropology; nay, it embraces the philosophy of all animated, nay, of all organic nature. On this account, therefore, it will not be possible for us to notice it in all its details. Moreover, it is by no means light reading. The author aims throughout at such strictness of metaphysical expression, that the reader, unless he has previously undergone the discipline which attaches to the perusal of such works as those of Hamilton and Kant, runs the risk of failing to understand what he readeth. For ourselves, we must confess, that after bestowing all the attention of which we are capable, and with all possible anxiety to be well informed on the subjects which Dr Laycock discusses, we have still found ourselves too frequently at fault as to a meaning in what he propounds.

Like all great works which have marked an epoch in philosophy, our author commences by a dissertation on Method. To this about a fourth part of the first volume is devoted, and as the author lays great stress upon this part in all that follows, we cannot do better than present our readers with a summary of his method in his own words, especially as that summary is also a programme of the whole work.

"*Summary of the Method* (vol. i. p. 113).—If, then, we consider the preceding doctrines, with a view to a practical development of our proposed method, we shall find that there are three stages or steps by which it may be carried into effect. First, we shall have to inquire into the general and scientific experience of mankind as to their states of consciousness (Empirical Psychology); next, we shall have to examine into the fundamental laws of existence (Ontology); and thirdly, into the first principles of mind as an ordering force to ends (Ideology, or mental dynamics). In the first, we examine consciousness in relation to vital phenomena; in the second, existence in relation to vital and physical phenomena; in the third, we develop the great correlations of mind with the physical and vital forces considered in relation to design in creation, viewed as a systematic unity, or the doctrine of ends. This will bring the highest manifestation of mind—as a creative and regulative power—into synthesis with creation, and consecutively into synthesis with the human mind. Here, the method will show that the ideas of the Divine mind, as revealed in the phenomena of creation, are none other than the fundamental ideas and *à priori* conceptions of the human mind, as revealed in consciousness; that the ends aimed at and attained by the Creator are the objects of the instinctive desires of the creature; and that consequently the phenomena of nature constitute a reflex of the human mind. . . . In establishing these principles, I shall show their general applications to metaphysics, or a science of the fundamental laws of thought; to biology, and the entire group of natural history sciences; and to sociology; and then proceed to develop more especially the scientific basis of a mental physiology and organology, and their bearings upon medical psychology and mental pathology. The whole will thus be a philosophical, scientific, and practical exposition of the fundamental laws of life and thought in their correlations. As such it will constitute a solid basis upon which the metaphysician, moral philosopher, political economist, biologist, zoologist, and medical practitioner can alike build up their respective departments; and at the same time be a starting-point for the man of general culture, who wishes to study human nature under all its multifarious aspects. Such a wide field of inquiry must necessarily be passed over cursorily; errors, too, are inevitable, from the very nature of the subjects considered; still, I indulge a hope that the views I shall set forth, however imperfectly, will contribute, in some degree at least, to the building up of a true philosophy on the solid basis of observation and induction, and be of practical use." (P. 115.)

Making allowance for the sanguine anticipations of the author, these remarks present Dr Laycock to us under a very favourable point of view; and they are in perfect keeping with his teaching in the University, which has this for its characteristic, that he constantly endeavours to direct the minds of his students to the phenomena and laws of mind, and the evidence of design in organisation, and not to rest satisfied, as medical students are ever prone to do, in the mere unfoldings which the scalpel and the forceps can effect.

Our author's method is at once inductive and deductive. It is also pre-eminently teleological, and that in a very large though somewhat peculiar sense. Thus, he regards mind as a regulative but not a constitutive principle in nature (p. 111). He also gives, as the very characteristic of mind, that in acting as an ordering force in nature, it works towards ends; but he considers it a mistake to expect that these ends shall always be good (112). He also often invokes his method, as if it were exclusively sensational or observational, and regards all that lies beyond the discovery of the senses, nay, the application of a test, as merely speculative; while at other times, as in the extract above, he goes so far in the opposite direction as to identify the *à priori* conceptions of the human mind with the ideas of the Divine mind; and therefore trustworthy, surely, if anything be. In a word, he does not refuse to acknowledge as his master Plato as well as Aristotle. Were we to attempt to characterise his method in a single term, we should, however, have to say that it was Baconian—but this not without reserve of a broad margin for Dr Laycock's own ideas, unfolded in detail in the first 115 pages of his work.

Of these the most characteristic, perhaps, is that which he designates by the name of "teleiotic idea." He grounds all nature upon teleiotic ideas; and in giving eminence in science to a chastened teleology, we fully sympathise with him. What he says is perfectly true—"the human mind can no more divest itself of the idea of design as a cause of phenomena, than it can divest itself of the ideas of cause, or space, or time" (vol. i. p. 108). Intelligence will never be satisfied, nor will it permanently award the name of philosophy to any form of speculation which ignores the end for which a thing exists. Nor is it possible to enter into the study of the structure of organised bodies, or to practise with success the healing art on any other principle. In this way alone can the step from the known to the unknown be made. The end, the function, in fact, is often fully ascertained, and indeed obvious, while yet the organ, in all those points which make the difference between a state of health and a state of disease, still lies wholly concealed from us. It is only by the way in which it does its work that the state of the organ can be judged; and let us operate upon it as we may, it is only by changes that we have induced in the function that we can learn anything generally speaking of the changes which we have effected in the organ. We therefore regard Dr Laycock as eminently in the right in laying so much stress on ends, and in viewing objects and arrangements in reference to the ends which they serve. What we wonder at is, that substantially, in the body of his work, he should build everything on teleiotic ideas merely, without allowing either himself or the reader to go a step deeper. These ideas are regarded by him as causes of the order of nature. They are undoubtedly objective in his esteem, and they are manifestations of mind. Why, then, does he stop short of admitting that which consciousness, which reason, which common sense affirms so constantly, that every quality or attribute, every function, motion, manifestation, idea, bespeaks a substance, being, or thing; or in our author's language, a substratum of which it is a manifestation? Postulating, as he does, teleiotic ideas continually, as at the very root of nature, and the cause of her existing order, why does he not admit, nay, immediately infer, that there may be (for there must be) an intelligent Being elsewhere and otherwise than in correlation with a brain, and our own brains may be dissolved, and yet possibly something of us survive, which shall be no stranger to itself, or to its God, or to the objects around it. Not but our author believes all this. What we grudge is, that he has framed his method so as to exclude the possibility of any of these things as scientific truths, and throws a burden upon faith which philosophy ought to assist in bearing. It is not possible to pronounce permanently a divorce between philosophy and religion. They are wedded together in the very nature of things. They stand or fall together. If one is denounced, the other soon withdraws. Our author, in common with many other physiological psychologists, may be very jealous of the interference of theology in science. But is he not, after all, a religionist in science himself? What, in fact, is his philosophy but an articulate expression, in the language and conceptions of Europe, of that profound imagination which constitutes the Budhistical religion? That religion is to the effect that the universe is pregnant with mind; that mind inworks everywhere, but attains to consciousness only in man and the species which have paved the way for his introduction on the ever-changing yet everlasting platform of existence. Hence, man, as the highest manifestation of mind, is to be worshipped, &c. Now, though our author has, towards philosophers and good men in general, only those benignant regards which are equally philosophical and Christian, and falls very far short of worshipping any man, nay, sometimes censures very severely men who are high in popular favour, as Dr Carpenter must confess; yet, bating this element, his system in other respects is wonderfully similar to that of the Eastern sage, Gautama Budhu (Gautama the Intelligent). And it says much for the profound reflectiveness of the East, that ages before the birth of Christ the philosophers of Asia had conceived and worked out systems of physiology, which we in the West, in these most modern times, when we adopt the same methods, can do no more than reproduce in other terms. Bacon and Descartes, Spinoza, Schelling and Fichte, Auguste Comte and our author, have all had their forerunners in ancient India.

But seriously, what, it may be justly asked, is Dr Laycock's philosophy, that which he himself refers to in the beautiful quotation that we have given from his work? To this we answer—though not without a caution to the reader, since we do not pretend to have mastered his philosophy in all its details—that it is a dynamic theory of the universe which carries the great idea of unity as far as that idea can possibly be carried. Unity is the first of our author's teleiotic ideas, and it has entire possession of his thoughts. As an existence, he postulates Force, but of one kind, and always conserved the same in quantity, though transformed from time to time, as it flows up or down the stream of being, manifesting itself now as physical, now as vital, now as conscious force. To this agent, when considered generally, he applies the term "mind" (p. 131); and of mind, he gives as the characteristic, that it can give motion (p. 124). Of thought, also, as a phenomenon of mind, of course there can be no doubt. Nor does our author seem to think that there is any difficulty in making the transition from motion to thought. In a word, nothing stands in the way of a grand law of unity, which, however, exists also under a law of differentiation, to which we owe all the highest products of organisation, and especially consciousness itself. This law of differentiation, especially as exemplified by the nervous system, our author illustrates very beautifully in the ascending scale of animal nature, and co-ordinates with it instinct, rising into sensibility and consciousness; with a curious appendix on unconscious cerebral action, in reference to which he feels called upon to vindicate his own discoveries.

And here, if along with the much that we have said in favour of our author's work, we have not scrupled to regard it as very defective, inasmuch as he loses the opportunity, which his own system supplies him, of affirming the existence of intelligence not necessarily in connection with nervation, we have again to lament that he loses the opportunity which his own view of mind gives for affirming the possible existence of liberty, and therefore of a moral system in the ordinary acceptation of the term. Mind, as we have seen, he regards as that which can begin motion—which mere matter cannot. Now, to begin motion is to act, in the proper sense of the term. And as the power to begin implies also the power of suspending, or of not beginning or of changing a motion once begun, or superadding a new beginning, or changing the first motion, we have here the conception of a power to which, if we add thought, we have not a bad idea of what is meant by a free power or will. If, then, along with much that savours of the soundest philosophy, we had found something of this kind in our author's volumes, we confess that we should have been highly gratified; for when we are told or left to infer that our consciousness of freedom, whether to choose or to act as we please, is a mere delusion, a self-deceiving, and a product of ignorance, which it is the part of science to dispel, we cannot but fear for the future of philosophy, nay, of humanity itself, so far as scientific speculation can affect it. In bygone times philosophers, no doubt, in estimating the individual, whether as to his actual attainments or his capacities, did underrate the influence of his organisation and his environments. But if, as a reaction against that underrating, we are now to be called upon to believe that man is altogether a creature of relation, merely a little pinion in the great wheel-work of nature, and that he cannot anticipate or go beyond his organisation one iota, this is a fearful retribution. Why may we not suppose, on the general principles of our author, that this mind which has the power of beginning motion should, after having been restrained in this respect by manifold mechanical and chemical bindings, and capable in the lower regions of nature only of conserving, continuing, and communicating motions impressed, ultimately, when accommodated with a brain as a dwelling-place, be so far set free, that consciousness affirms the truth when it affirms the liberty of mind? It is certain that, when compared with all other structures, that of the brain is singularly loose, and its cells curiously isolated. It also decomposes and falls to pieces with singular facility. And if we were to say of the entire nervous system that it was a manifestation of mind, might we not say of its central, its most tenuous part, that it is a manifestation of mind at last individualised and emancipated again from mechanical and chemical, and all other binding and blinding agencies, so that it was to a certain extent free? If so, then to this centre of force or essence there would emphatically attach the term "mind," as ordinarily used; or, in the language of Leibnitz, that of "monad." Moreover, such a view would not only satisfy the requirements of cerebral physiology, but would explain very felicitously the curious phenomena of unconscious cerebral action on which our author justly lays so much stress.

Thus, let the mind be a principle such as Leibnitz conceives it to be, a centre of force, an essence, a monad, which is a mirror of the universe from its own point of view, and let its womb, cradle, dwelling-place, in the present form of our being, be in the centre of our nervous system, the *locus niger*, or elsewhere, or, more generally, let it live wherever the nervous system culminates, then such a mind may be expected, when existing normally in its relationships, first, to mirror generally the laws of the universe, so as to give such cosmical intuitions as the mind of man is known actually to possess; and, secondly, to mirror specially, from moment to moment, the action going on in its cerebral environments, so as thus to give all that intimate dependence of the states of consciousness upon those of the brain which physiology affirms. By such appliances as closed or clouded the mind's-eye, all those processes and actions of the organism to which consciousness is normally a witness, would become merely mimetic and automatic—there would be unconscious cerebral action. By such applications, on the other hand, as opened the mind's-eye or lighted it up, there might be produced those states of exalted perception and sensibility of which man is known to be occasionally capable. If, in short, in accordance with the Catholic philosophy of all ages, we regard brain as one thing and mind as another, both of them finely co-ordinated and placed in mutual dependence in the present form of our being, we are able to explain the phenomena of psychology and mental pathology far better than if we assume that consciousness is merely the last and highest term in a series of dynamic phenomena which first manifest themselves in inertia and gravitation.

The different bearings of these two views on the destiny of man is too obvious to require to be stated.

It may indeed be said, that the view which we have opposed to that of our author is merely a speculation—and that is true; but our author's view is merely a speculation also. In the entire field, nothing but speculation is possible. The products of inquiry therein can never lay claim to a higher name than that of hypothesis. They are not on that account, however, to be neglected or undervalued. And science owes much to Dr Laycock for having elaborated, in a most difficult walk, a work of singular comprehensiveness, and not only favoured the public with his own views, but given also a very extensive survey of the views of others.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 6th February 1860.—SIR DAVID BREWSTER, Vice-President, in the Chair.

The death of General Sir Thomas Makdougall Brisbane, Bart., G.C.B., President of the Society, was announced, as having taken place at Brisbane on 27th January; thereupon the following motion was made by Lord Neaves, one of the Vice-Presidents of the Society, and unanimously agreed to:—

"That the Society, at this its first meeting after the death of Sir Thomas Makdougall Brisbane, should place upon record the expression of its deep regret for that event, and its high estimate of the character of Sir Thomas Brisbane, who, besides other eminent public services, was during a long life conspicuous as a sincere lover and active promoter of science, and who so worthily presided over this Society for a period of twenty-seven years.

"That an excerpt from this minute be sent to Lady Makdougall Brisbane, with an expression of the Society's sympathy and condolence."

The following Communications were read:—

1. On the Capture of Whales with the aid of Poison. By Dr Christison.

(This paper appears in the present Number of this Journal.)

2. Notice regarding the Branchial Sac of the Simple Ascidiæ. By Andrew Murray.

In a paper which I read last year before this Society, "On the Structure and Functions of the Branchial Sac of the Simple Ascidiæ," I stated that I had fed and injected ascidiæ with indigo and other coloured sea-water, and that in those so fed, the coloured material was never found on the exterior of the sac, but always deposited on the inner wall, and that injection by the mouth into the sac failed to push the coloured matter through its walls, except by rupturing them.

Since I read that paper, I have in two instances found, on feeding the *Ascidia virginea* with indigo, that this colouring matter passed through the windows of the sac and was partially deposited on each side of the sac—some of it sticking in the meshes; and I hasten to correct the erroneous impression conveyed by the negative instances in my former paper.

Another correction which I wish to make is this: I stated that Dr Wright and I thought that, under a high power of the microscope, we saw a diaphanous membrane stretching across the branchial stigmata, which showed a polygonal structure similar to that of epithelica. I have never been able to detect this again; but I have seen something approaching to it, which I am satisfied was a compressed aggregation of blood-globules, and I strongly suspect (particularly since finding that the indigo has passed through these stigmata) that my former observation is to be referred to some deceptive appearance of this kind.

Monday, 20*th February* 1860.—Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Action of Uncrystallised Films upon Common and Polarised Light. By Sir David Brewster, K.H., F.R.S.

Since the discovery of the polarisation of light by refraction, the action of a pile of transparent plates upon common and polarised light has not been studied by any of the writers on physical optics. It was believed that a pencil of common light was completely polarised in the plane of refraction when the plates were sufficiently numerous, no special notice having been taken of the light thrown back by reflexion into the transmitted and polarised beam. Sir John Herschel, indeed, had referred to it; but he remarks that "it mixes with the transmitted beam, and, being in an opposite plane, destroys a part of its polarisation."* So long ago as 1814, Sir

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* Treatise on Light, Art. 868.

David Brewster had shown that this reflected light is distinctly visible as light polarised by reflexion;* but owing to the difficulty of procuring very thin plates of glass with perfectly parallel surfaces, it was impossible to ascertain the true character of the oppositely polarised pencils.

Having obtained, however, films of decomposed glass of great thinness, and perfectly colourless, the author was enabled to prove that the transmitted beam consisted of two pencils oppositely polarised, and that when polarised light was incident obliquely on such a pile, and subsequently analysed, the pile of films exhibited all the properties of a plate cut perpendicular to the axis of a negative uniaxal crystal, the tints produced by the interference of the pencils rising to the *blue* of the second order of Newton's scale of colours, by increasing the obliquity of the incident pencil.

A line perpendicular to the plates or films at the point of incidence corresponds with the axis of the uniaxal crystal; and the different azimuths in which the polarised ray may be inclined to this axis correspond with the principal sections of the crystal.

When the films of decomposed glass are circular spherical segments and colourless, the black cross and its accompanying tints are finely displayed, as in the system of rings seen along the axis of uniaxal crystals. When the films have the colour of thin plates, and are deeply spherical segments, the tints of the rings which accompany the black cross are singularly modified.

2. On Mr Darwin's Theory of the Origin of Species. By Andrew Murray.

Monday, 5th March 1860.—Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Utmost Horizontal Distance which can be Spanned by a Chain of a given Material. By Edward Sang, Esq.

The method of investigation followed in this paper for finding the limit for a uniform chain, is to suppose a multitude of catenaries all having a common vertex, and to take, in each of them, that point at which the tension is fixed. The curve passing through all of these points has for its equation—

$$z = s\,(1 - \sin\theta),$$

$$x = s\ .\ \sin\theta\ .\ \text{nep log cot}\ \frac{\theta}{2},$$

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* Phil. Trans., 1814, p. 226.

in which s is the tension, z the vertical, and x the horizontal ordinate.

This curve, a correct figure of which was exhibited, has its extreme horizontal limit when $\theta=33^\circ, 32', 03''$, from which the utmost horizontal span of a uniform chain is found to be $s \times 1{\cdot}325\,4838$, the height or versed sine being $\times 447\,5659$, and the length of the chain $s \times 1{\cdot}667\,1130$ when s is taken equal to the modulus of strength of the material.

This, however, does not show the absolute limit; for, by reducing the thickness of the chain at each point till it be just able to bear the strain to which it is subjected, we shall remove all redundant weight. The form which such a chain assumes may be called the catenary of regulated strength.

Its equations are—

$$i=\frac{x}{s}$$

$$z=s \text{ . nep log sec } \frac{x}{s}$$

$$l=s \text{ . nep log tan } \left(\frac{\pi}{4}+\frac{x}{2s}\right)$$

i being the inclination of the curve to the horizon. This curve has two vertical asymptotes placed at the distance πs from each other, which distance is thus the absolute extreme horizontal span that can be reached by a chain having s for its modulus of strength. A correct drawing of this curve was also shown.

2. On the Climate of Edinburgh for Fifty-six Years, from 1795 to 1850, deduced principally from Mr Adie's Observations; with an Account of other and earlier Registers.—On the Climate of Dunfermline, from the Registers of the late Rev. Henry Fergus. By Professor J. D. Forbes.

The paper on the Climate of Edinburgh is divided into seven sections.

The *First Section* includes an account of the earliest records of the thermometer at Edinburgh which are to be found in the *Edinburgh Medical Essays.* They date from 1731. They were made with a thermometer having an arbitrary scale, which is described in *Martine's Essays*, and they appear to have been recorded with much care; but they cease in 1736. The next series, printed in the *Essays of the Philosophical Society of Edinburgh*, commence in 1764, and are continued till 1770. A parallel Register was begun at Hawkhill, near Edinburgh, and continued till 1776, and probably later. Professor Playfair's Observations (printed in the Royal Society Transactions) only supply one year (1794) of the interval which elapses before we enter upon the elaborate Register kept by the late Mr Adie and his family.

Second Section. Mr Adie's Observations were continued from 1795 to the middle of 1805. They then ceased until the year 1821, after which they were steadily pursued, with the assistance of different members of his family, until 1850. In order to supply the missing years 1805–1820, the author was fortunate enough to recover (through the kind agency of Mr David Laing) a Register kept at Dunfermline for above thirty years by the late Rev. Henry Fergus, whose son, the Rev. John Fergus, kindly lent the Register, and allowed him to make use of it.*

The history of this Register and its results are given in a short separate paper. The climate of Dunfermline has a very close approximation in character to that of Edinburgh, not only as regards the mean annual temperature, but also as to the distribution of heat at different seasons.

By availing himself of the Dunfermline Register for the years 1805–1820, and using simple reductions, the author is enabled to estimate with considerable confidence the mean temperature of each year, and each month of each year, from 1795 to 1850, at Edinburgh.

The *Third Section* contains the monthly means of the entire series in a tabular form. The highest mean annual temperature (49°·60) was that of 1846, the lowest (44°·44) that of 1799. The mean of the whole period, deduced from nearly 35,000 observations, was 46°·77, or, excluding the Dunfermline observations, 46°·88. A series of Tables is also given, classifying the seasons according to the annual range, and by the temperature of the hottest and coldest months respectively. The hottest month was July 1808, the coldest, January 1814. In 56 years June was 5 times the hottest month, July 36 times, and August 15 times. November was twice the coldest month of winter, March 1½ times, February 10½ times,† December 15, and January 27 times. A classification of the years according to the earliness or lateness of the greatest summer heat is next given.

Section Fourth contains the monthly and annual fall of rain from Mr Adie's observations, viz. from 1795 to 1804, and from 1822 to 1849. The mean annual fall is exactly 25 inches, ranging from 36·60 in 1795 to 15·27 in 1826. The distribution of rain in the different seasons is then given, being greatest in summer and least in spring.

In *Section Fifth*, the author has considered whether any law can be traced in the succession of the seasons throughout the period embraced by these observations. But beyond the fact, that hot and cold years usually occur in groups of from 7 to 12 years' duration, nothing definite can be deduced from the Tables. Conformably with this remark, there is a slight appearance of maximum temperatures occurring about the years 1809, 1829, and 1849; and of minima in 1799, 1819, 1839.

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* Mr Fergus has, since this paper was read, presented this interesting MS. register to the Royal Society.

† When the temperature of February and March was the same, as in 1807, then one-half is the proportion for each.

Section Sixth is on the form of the Annual Curve of Temperature and its Fluctuations. Adopting the usual mode of notation by the sines of arcs, and making the given date reckoned from January 0 (the extent of the year being denoted by 360°), we have for the temperature, y, of the given epoch x.

$$y = 46^\circ{\cdot}88 - 10^\circ{\cdot}98 \sin (x + 68^\circ\ 28') + 0^\circ{\cdot}96 \sin (2\,x + 22^\circ).$$

The average temperature of each day of the year being taken for the 40 years of Mr Adie's observations, and being projected in a curve, is well represented on the whole by the preceding formula. It is plain, however, that even 40 years is by much too short a period to give with accuracy the mean temperature of any given day. The following Table contains the mean temperature of each day of the year, founded on Mr Adie's observations.

In this section are farther considered the inflections or "periodic anomalies" of the annual curve. When the normal curve deduced from the preceding equation (p. 295) is projected and compared with that which the projection of the daily temperatures produces, the latter is found to fluctuate considerably, and to pass in an irregular manner, sometimes above, and sometimes below, the geometric curve, which, however, fairly represents the totality of the observations.

We find, however, that there are deviations from the average or normal curve, which occur at certain seasons, and which do not appear to be entirely accidental. Several of these may be traced, for example, in each of the four decennial periods into which the series may be divided. These are called "periodic anomalies;" and their occurrence has previously been announced by De Humboldt, Arago, M. Quetelet, and others.

The most conspicuous of these anomalies occur in December, January, and February. An irregular elevation of temperature usually happens in the two middle weeks of December, followed early in January by an accession of cold, which accelerates the epoch of lowest temperature by at least a week, when we compare it with the geometric curve. This is again succeeded, in the latter part of January and beginning of February, by a period of comparative warmth. These anomalies seem to obtain at least over a great part of the west of Europe.

The author gives the name of "fluctuation" to the variation in the temperature of a given day of the year, from one year to another, arising from causes purely local and temporary, or, as we may call them, accidental. By applying the calculus of probability to the forty years' observations, we might assign the "probable uncertainty" in the determination of the temperature of any given day. The author has, however, confined himself to noting the highest and lowest mean temperature on a given day which has occurred during 40 years. These differences are sometimes very large. But they vary from one season to another according to a well-marked law. The "fluctuation" is greatest in January, when it amounts to 28° or 29°, and least in July, when it is only 16° or 17°. On the contrary, the *diurnal* range or difference of the maximum and minimum reading in 24 hours is least in December (9°·5), and greatest in June (18°).

Table showing the Mean Temperature of every day of the Year, at Edinburgh, from an Average of 40 *Years' observations.*

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
	°	°	°	°	°	°	°	°	°	°	°	°
1	36·1	37·2	39·1	42·5	48·5	53·6	57·1	58·4	56·0	51·2	45·1	39·7
2	35·2	37·2	39·2	43·1	47·9	54·5	57·2	58·3	56·9	50·9	44·9	39·4
3	36·0	37·5	40·0	42·4	47·9	53·9	57·3	57·6	55·6	51·1	43·4	39·2
4	37·5	36·9	38·9	42·5	48·1	54·2	58·2	58·0	55·2	50·4	41·7	39·1
5	36·7	37·2	38·4	43·5	48·6	54·1	58·3	57·6	54·7	49·8	42·1	38·5
6	36·4	36·6	38·4	43·5	47·9	54·3	57·7	58·2	55·1	49·6	42·3	38·7
7	36·2	37·2	39·0	44·2	48·5	54·7	57·4	58·6	55·1	50·1	42·6	39·7
8	36·0	38·2	39·4	44·2	48·6	54·6	57·8	59·2	54·5	49·1	42·1	39·2
9	35·4	38·5	40·3	43·4	47·7	54·4	57·2	58·5	54·9	48·6	41·9	40·2
10	35·3	38·6	40·5	43·4	47·9	55·1	58·2	58·2	54·6	48·7	42·3	40·1
11	34·8	38·5	40·3	42·6	48·4	55·6	58·3	58·6	54·6	48·7	42·2	40·0
12	35·5	38·3	40·3	43·9	48·4	55·5	58·7	58·4	53·7	47·0	41·6	39·7
13	36·4	37·7	40·6	44·2	48·7	56·3	59·0	57·8	54·2	46·8	42·0	39·7
14	36·1	38·5	40·9	45·4	48·3	55·8	59·0	57·2	54·1	47·8	41·3	39·3
15	37·1	38·3	41·2	45·7	49·1	55·9	58·6	57·5	54·6	47·4	40·6	40·2
16	36·1	38·3	40·7	45·3	50·7	56·1	59·0	56·9	55·7	47·5	40·9	40·5
17	36·1	37·9	40·9	44·8	50·7	56·4	58·6	57·4	55·1	47·5	41·7	39·6
18	37·9	37·4	41·4	45·0	50·7	56·2	58·4	58·2	53·5	46·5	39·8	39·0
19	38·0	37·9	41·5	45·6	50·3	55·4	58·3	58·1	53·2	47·1	40·4	39·0
20	37·0	38·0	41·4	45·7	51·0	56·2	57·9	57·6	52·2	47·7	40·8	38·1
21	37·3	37·1	41·7	45·3	51·5	56·8	57·9	56·9	52·2	46·5	41·0	38·1
22	37·0	38·8	41·6	45·8	52·0	55·9	58·4	55·7	51·6	46·8	40·3	38·5
23	38·1	38·9	40·9	45·3	52·5	56·7	58·3	57·0	52·0	46·3	39·9	36·5
24	38·1	39·0	40·6	45·1	53·2	56·3	57·6	56·2	52·5	46·0	39·7	37·0
25	37·6	38·2	41·0	45·9	52·4	56·2	58·5	56·5	52·4	45·5	38·4	37·7
26	38·1	37·8	40·9	47·0	52·7	56·2	58·7	56·1	52·5	44·2	39·5	37·0
27	36·9	38·2	42·7	46·8	53·0	57·0	59·4	56·5	52·2	45·0	39·1	36·1
28	37·4	38·3	42·1	47·3	52·5	57·4	59·8	56·0	51·4	44·1	39·0	35·5
29	36·6	39·4	41·5	47·5	53·3	57·3	58·4	56·8	50·6	43·8	40·1	36·7
30	37·7	......	40·9	48·1	53·4	57·2	58·1	56·6	51·0	44·9	39·5	37·9
31	36·9	......	42·5	......	53·9	......	59·0	55·9	......	44·8	......	36·8

Section Seventh.—The author concludes the paper with a comparison between the meteorological character of the seasons for fifty-six years, and the price of oats in the Edinburgh market for the same period, which was obligingly furnished to him by Mr Lawson. He has been quite unable to trace any connection between these classes of facts, and he recommends the subject to the consideration of those who are now occupied in considering the bearings of meteorology upon agriculture. When the seasons from 1795 to 1850 are arranged according to the price of oats, their order bears no intelligible relation to one or any of the previous classifications of those years according to warmth, moisture, or earliness. Thus much appears from the inquiry, that the attempt of Sir William Herschel to deduce the climatic influence of the solar spots from the market price of wheat, rests on a fallacious basis. It is only fair to add, that Sir William Herschel employed it with a reservation as to its reliability as a criterion of the temperature.

3. On the Mountain Limestone and Lower Carboniferous Rocks of the Fifeshire Coast. By the Rev. Thomas Brown. Communicated by Dr Allman.

The writer stated that this paper was the result of observations made while staying at the sea-coast for a few weeks in autumn. Near Ardross a bed of limestone had been discovered, with peculiar fossils, which promised geological results of some interest. To determine its stratigraphical position, it was necessary to reduce to order a portion of the coast, hitherto held to be in a state of hopeless confusion. This led to fuller inquiries, till the rocks underlying the coal-field had been examined from Burntisland to St Andrews.

Section I. A general description of the rocks was given, as seen along the shore, accompanied by a section in which they were laid down to scale.

Section II. Two classes of trap-rocks were referred to, viz.—1. The contemporaneous and interstratified; 2. The intrusive—this term being designedly used as expressing no opinion in regard to their origin, merely that the surrounding strata had been fractured, and through these fractures the traps in question had come into their present position. No proof had presented itself that these intrusive traps had exerted an upheaving agency, except perhaps at one point, and there only to a small extent.

Section III. The Mountain Limestone was described as consisting of three parts, viz.—

1. The six upper limestones, A to F. These, with their intercalated strata, immediately underlie the coal-fields. They are marine, and to them the term Mountain Limestone has usually been restricted. Their fossils were described.—Of crustaceans, there were one species of Trilobite, one of Eurypterus, one of Gampsonyx,

(a genus not hitherto found in Britain), and two of Dithyrocaris, both, it is believed, new. Of fish, besides Rhyzodus, Holoptychius, &c., there were two new species, a Cochliodus and a Ctenacanthus. The plates also of a tuberculated fish, belonging either to Pterichthys or some allied genus, were exhibited, proving that this great class had passed far up into the carboniferous system.

2. Estuarine strata, between F and L. These comprise the well-known Burntisland limestones, corresponding with beds west of Pittenweem.

One remarkable jaw of a pycnodont fish was exhibited from this part of the series near Kilwinning. Only one example of this class of fish-remains had previously occurred in the whole Palæozoic system—a small jaw found near Leeds.

3. The limestone L, the line of Lower Encrinites. This occurs east of Pittenweem, at Crail and St Andrews. The fossils were described similar as a whole to those of the beds A F.

Forty species of fossil shells occurring in these rocks were enumerated, only twelve of which are given in Professor Nicol's list of Scottish fossils. Among those here added, were *Sanguinolites tricostatus*, *Chemnitzia gracilis*, *Murchisonia trilineata*, &c. &c.

Section IV. Lower Carboniferous. This great underlying series was described in its leading features, especially—1. The Myalina beds—limestones composed of a single bivalve allied to Unio. 2. The deep-sea character of the fossils found at different levels, species of Orthoceras, Natica, Lingula, &c. 3. A limestone charged with great abundance of a new annelid, a species of Spirorbis, beautifully curved in a serpentine form.

Section V. Results. The two groups.

The writer explained that he had been led to deviate from the usually received classification of these rocks. Taking the upper portion of what Mr Maclaren terms the calciferous sandstones down as far as the bed L, and adding these to the upper zone, usually called the Mountain Limestone, two well-marked groups would be formed, and a well-defined line of division obtained.

1. *The Mountain Limestone.* The bed L, containing the same fossils with the six upper limestones, must (notwithstanding the intervening Estuarine beds) be classed along with them, and forms the base line of the upper group. The whole strata, from A to L, are approximately about 1000 feet in thickness. The limit upwards was not examined. The fossils all belong to the Mountain Limestone, and are extremely characteristic.

2. The underlying group, the Lower Carboniferous, is characterised by—1. The great Myalina beds. 2. The relative abundance of Cyclopteris; the Sphenopteris being the specially characteristic plant of the upper group. 3. The circumstance that the carboniferous fauna occurs only in an incipient state. The few species of Orthoceras, Aviculo-pecten, &c., which do occur, are of a carboniferous

character, and prove the presence of deep-sea conditions; but all that specially marks the Mountain Limestone fauna (the Encrinites, Corals, Brachiopods) are either absent, or occur very scantily.

It would thus appear, that in this great series, which exhibits with singular clearness the development of the carboniferous epoch, the bed L marks the point of time when the Mountain Limestone fauna in its full strength came into view, while the lower carboniferous shows the same fauna in an incipient state. The latter may be studied with singular advantage on these shores, especially from Fife Ness to beyond Kingbarns.

Monday, 19th March 1860.—Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On an Apprehended Depreciation of Money, arising from New Supplies of Gold. By Patrick James Stirling, F.R.S.E.

2. On the Chronology of the Trap-Rocks of Scotland. By Archibald Geikie, F.G.S., of the Geological Survey of Great Britain. Communicated by Robert Chambers, Esq.

The first part of this paper contained a review of the existing nomenclature of trappean-rocks, and the following arrangement was given as that which the author had found to be most useful for practice in the field:—1*st*, Ash and volcanic conglomerate. 2*d*, Interbedded augitic traps (greenstones and basalts). 3*d*, Interbedded felspathic traps (felstones, porphyries). 4*th*, Intrusive augitic traps. 5*th*, Intrusive felspathic traps. By adopting a different colour for each of these classes, the general relations of an intricate trappean district could be shown at a glance.

Although it was well known that the trap-rocks of Scotland belonged to several distinct geological epochs, much still remained to be done, both in determining their exact age and in working out the details of their structure. The author had been engaged in this subject for several years, and the present paper was intended as the first of a series elucidatory of Scottish trappean geology.

Silurian.—It was remarked that, both in the Lower Silurian grits of the Lammermuirs, and in the Upper Silurian grits of the Pentlands, there is an abundance of felspathic matter, pointing to the existence of felspathic rocks, either then or previously ejected. The Lammermuir chain is likewise traversed by innumerable felstone dykes, probably produced at the time of the folding of the Silurians. At Reston, in Berwickshire, beds of ash occur in the Lower Silurian.

Old Red Sandstone.—The author referred to a previous paper (read before the Geological Society) in which he had shown that the

Old Red Sandstone of the south of Scotland consists of two distinct portions—one conformable with the Upper Silurian, and traversed by the same foldings and dykes; the other lying utterly unconformably, both on the Upper Silurian and the Lower Old Red. The igneous rocks of the older series probably occur in the Sidlaw Hills; those of the newer series are well displayed in the Pentlands, the structure of which was detailed.

Carboniferous.—The great abundance, the variety of character, the local nature, and the great vertical range of the carboniferous traps of central Scotland, was shown by a detailed sketch of the geology of the Lothians. The range of hilly ground between Bathgate and Linlithgow was pointed out as an eminently characteristic district. A careful survey had shown that no well-marked zone of the 6000 or 8000 feet of carboniferous strata in some part of the Lothians was without traces of contemporaneous igneous eruptions.

Oolitic.—The structure of Skye and Raasay was described. These islands (when examined along a section from Dun Can, in Raasay, to Dunvegan, in Skye) consist of successive sheets of greenstone, with intercalated seams of estuary limestone, shell, sandstone, and coal, belonging to the Oolitic series.

Secondary, or Tertiary.—Allusion was made to the Tertiary basalts of Mull, and to the possibility of there being other igneous rocks of that age on the mainland. A section of Arthur's Seat was exhibited, showing a series of volcanic eruptions, resting quite unconformably upon some of Carboniferous date. This later group must be greatly posterior to that below; and the author collected evidence to show that it may be regarded as later Secondary, or older Tertiary.

Letter from the Rev. Dr Livingstone, F.R.S., to Dr Lyon Playfair, C.B.

River Shire, 28*th Oct.* 1859.

My dear Dr Playfair,—We left England in April 1858, and up to this time we have not received a single private letter from home. This saves me the trouble of apologising to any of my friends whom I have neglected. So here goes into the middle of things. We have just traced this river up to its point of emergence from the hitherto undiscovered Lake of Nyassa or Nyinyessi. This discovery is of more importance than at first sight appears, for it opens a cotton-field superior, I imagine, to the American, inasmuch as there are no frosts to endanger or cut off the crops; and instead of the unmerciful toil required to raise the staple there, one sowing of foreign, probably American seed, introduced into several parts by the natives themselves, serves for three years' crops. Even when burned down, the plants spring up fresh again. It may have disadvantages to counterbalance these points in its favour, but of these I am at present ignorant. There is a good day's channel from the sea at *Kongone* harbour up to Murchison's Cataracts in lat. 15° 55′ south. We have then only 33 miles of cataracts, past which a common road could easily be made—and the Shire itself is again navigable right into Nyassa in lat. 14° 55′ south. Above the cataracts the land is arranged into terraces east of the river. The lower or Shire Valley is about 1200 feet high, and exactly like the valley of the Nile at Cairo, only a little broader. The second terrace is over 2000 feet and three or four miles broad; the third over 3000, or about equal to Table Mountain at the Cape (long spoken of as the highest in South Africa). We travelled in the hottest season of the year, or that called in the West the "smokes," when, from the burning of tens of thousands of acres of tall grass, the atmosphere becomes like a partial cloud or fog, but insufferably hot. When we ascended the second terrace, the air felt delightfully cool, and on the third it was perfect, neither too cold nor too hot. All these terraces are wonderfully well supplied with running rills of deliciously cool water; and cotton of the indigenous variety (which feels more like wool than cotton, and requires to be cropped annually) is cultivated to a very considerable extent. On the last terrace rises Mount Zomba, which we ascended, and found to be in round numbers 7000 to 8000 feet high. Here it was cold; but there is cultivation and a fine stream in a large valley on its top. It has a base of 20 or 30 miles. We have thus differences of climate within a few miles of each other. This for keeping Europeans well. Then we are indulging the pleasant belief, from which you may deduct a percentage, that we can cure the fever, even in the lowlands, quickly and without loss of strength to the patient. We shall beat Holloway yet! only we tell every one what our pills are made of. It is the system followed when I was alone, and adverted to at the end of my book. We have, thank God, not lost a man yet, and gave the quinine a fair trial. It never prevented an attack. We have given it up now. We took it after our wine was done, partly for the sake of the dram, and partly to prevent you folks blaming us after we were dead.

Well, beyond Zomba the land between Shire and the two lakes of Shirwa, or, as its name really is, Tamandua, and Nyassa or Nyinyessi, contracts into a narrow isthmus, and all the slave trade from the interior must cross, in order to get past the lakes without embarking on either. We met a large slaving party there. I think they are what people suppose to be Arabs at the Angoxia river, but they could not speak Arabic. They were the most blackguard-looking set I ever saw. When they understood we were English, they made off by night, probably with the same opinion of us as we had of them. They had an immense number of slaves. A station for lawful commerce here would root out that traffic.

The lake at its southern end seemed eight or ten miles broad—had a heavy swell on it, though there was no wind—and it must be large to give off the Shire constantly (80 to 120 yards wide, 2 fathoms deep, and 2½ knots current) the whole year, with a variation in the river of about 2 feet from the wet to the dry season.

We should have explored it, but had left Mr Macgregor Laird's vessel in a sinking state. Funnel, furnace, deck, and bottom, all became honeycombed simultaneously. . . .

DAVID LIVINGSTONE.

Monday, 2d April 1860.—Lord NEAVES, Vice-President, in the Chair.

The following communications were read:—

1. On the Solidification of Limes and Cements. By George Robertson, C.E.

2. On Zinc-Methyl. By J. A. Wanklyn, Esq.

Considerable difficulties attend the preparation of zinc-methyl. Frankland, who discovered the body, obtained it by heating pure iodide of methyl and zinc enclosed in small glass tubes. Owing to the high temperature at which reaction takes place, much gas is formed; hence the operation must be confined to very small quantities of materials.

No determination of the boiling-point, specific gravity, nor yet of the vapour density of zinc-methyl, was made by its discoverer; from which fact may be inferred how small was the product available for investigation.

Frankland[*] has recently endeavoured to improve the process of preparation. He has tried a modification similar to that which he had introduced for zinc-ethyl. He mixed ether with the iodide of methyl, and heated with zinc in his copper digester. By this means ready decomposition of the organic iodide was obtained, and very little gas was evolved; but subsequently it was found impossible to separate the ether from the zinc-methyl. By this process Frankland did not succeed in obtaining any pure zinc-methyl.

To meet this difficulty is the object aimed at by the author of the paper.

Instead of using ether to mix with iodide of methyl, the author uses either a strong solution of zinc-methyl in ether, or pure zinc-methyl; either of which he has found capable of rendering the action of zinc upon the organic iodide easy, and unaccompanied by much gaseous products. All his digestions he makes in glass tubes heated in the water-bath to 100° C. The strong solution of zinc-methyl was obtained, in the first instance, by digesting together ether, iodide of methyl, and zinc, and afterwards distilling. The distillate was then employed in a second operation in place of ether, and so a still stronger solution of zinc-methyl resulted.

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[*] Annalen der Chemie u Pharmacie von Liebig, cxi. p. 62. Frankland.

By repeating the process zinc-methyl was finally obtained in a state of tolerable purity. A single tube, which had undergone four digestions, furnished about half an ounce of product, which analysis showed to consist of zinc-methyl nearly pure.

A determination of the vapour density of zinc-methyl made by Gay Lussac's method gave 3·291. The calculated theoretical number is 3·299. Accordingly, the condensation of zinc-methyl corresponds to that of ether, and not to that of hydride of methyl—in this respect resembling zinc-ethyl.

In addition to the properties of zinc-methyl mentioned by Frankland, viz. its extreme inflammability, its action upon water, &c., the author has observed that the body may be heated to 200° C. without decomposition. At 270° C. it begins to yield metallic zinc.

The boiling-point of zinc-methyl lies between 50° C. and 60° C., but the author reserves the accurate determination until he shall be in possession of several ounces of the pure body.

In one respect the author's observations do not accord with Frankland's; he has not been able to verify what has been advanced concerning the poisonous nature of zinc-methyl fumes.* Having performed more than a dozen distillations of the body, and having been much exposed to the fumes, he has not been able to mark any special effect upon his health. With regard to the disagreeable odour of the body, he has also to remark, that a mixture of zinc-methyl with much ether is more offensive than zinc-methyl pure, or containing only a little ether.

The author remarks, that considerations drawn from the state of condensation of the so-called organo-metallic bodies, lead to the conclusion that the metals are not the representatives of hydrogen. The formulæ of equal volumes of the following bodies reduced to a state of vapour are adduced as examples:—

Zinc-methyl.	Zinc-ethyl.	Mercury-methyl.	Mercury-ethyl.
$\left.\begin{array}{l}C_2\,H_3\\C_2\,H_3\end{array}\right\}Zn_2$;	$\left.\begin{array}{l}C_4\,H_5\\C_4\,H_5\end{array}\right\}Zn_2$;	$\left.\begin{array}{l}C_2\,H_3\\C_2\,H_3\end{array}\right\}Hg_2$;	$\left.\begin{array}{l}C_4\,H_5\\C_4\,H_5\end{array}\right\}Hg_2$.

Distann-methyl.	Distann-ethyl.
$\left.\begin{array}{l}C_2\,H_3\\C_2\,H_3\\C_2\,H_3\\C_2\,H_3\end{array}\right\}Sn_2$;	$\left.\begin{array}{l}C_4\,H_5\\C_4\,H_5\\C_4\,H_5\\C_4\,H_5\end{array}\right\}Sn_2$.

Hydride of methyl.	Hydride of ethyl.	Methyl-ether.	Ethyl-ether.
$\left.\begin{array}{r}C_2\,H_3\\H\end{array}\right\}$;	$\left.\begin{array}{r}C_4\,H_5\\H\end{array}\right\}$;	$\left.\begin{array}{l}C_2\,H_3\\C_2\,H_3\end{array}\right\}O_2$;	$\left.\begin{array}{l}C_4\,H_5\\C_4\,H_5\end{array}\right\}O_2$.

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* Liebig's Annalen der Chemie u Pharmacie, lxxi. p. 214. Frankland.

From which it appears that there is twice as much methyl in the standard volume of zinc-methyl vapour as there is methyl in the standard volume of hydride of methyl. It appears, also, that Zn_2 in zinc-methyl represents O_2 in methyl ether. There is no organo-metallic compound known in which the metal has a condensation corresponding to that of hydrogen.

3. Notes of the Dissection of a Female Beaver. By John Cleland, M.D., Demonstrator of Anatomy, University of Edinburgh. Communicated by Dr Douglas Maclagan.

(This paper appears in the present number of this Journal.)

4. On the Thyroid Gland in the Cetacea; with Observations on the Relations of the Thymus to the Thyroid in these and some other Mammals. By William Turner, M.B. (Lond.), Senior Demonstrator of Anatomy, University of Edinburgh. Communicated by Professor Goodsir.

The author, in the first instance, directed attention to the discrepant statements of various comparative anatomists respecting the thyroid gland in the Cetacea, quoting from the writings of John Hunter, Meckel, Cuvier, Carus, and Dr Martyn. He then related the result of his own dissections made on three specimens of the common porpoise (*Phocæna communis*), one being a fœtus, another a well grown male, the third an adult male. In each of these animals a well-marked thyroid gland was found, lying on the anterior and lateral surfaces of the trachea at its upper end, and extending slightly upwards on each side over the outer surface of the cricoid cartilage. It presented no division into two lateral lobes, as described by Cuvier and Carus, but consisted of a single uniform mass extending across the middle line. In the adult animal, which was examined in the fresh state, the other specimens having been some time in spirits, the gland presented a dark purple tint, and a soft and somewhat succulent aspect. Both in the fœtus and well-grown animal, the thymus gland was exceedingly well developed. A detailed description of the position and relations of this gland was then given, the long ascending processes which pass upwards, by the side of the great vessels, as far as the thyroid gland, being especially pointed out. These processes were intimately connected with the lateral portions of the thyroid by cellular tissue, but were not continuous with them.

The author next referred to Mr Simon's description of the thymus in the fœtal dolphin.

The microscopic characters of the thymus gland in the well-grown porpoise were then given. It was found to consist of small closely packed corpuscles, about the size of, or a little larger than, the red corpuscles of human blood—its structure, in fact, exactly corresponding with that which is familiar to us in the fœtal gland. Thus this animal gave us an additional illustration of the truth of the statement made by Haugsted and Simon, that the thymus is not merely a fœtal structure, but that it plays an important part in the animal economy for some time after birth.

The author considered that the close relation which was found in these porpoises between the thymus and thyroid glands, might be regarded as confirmatory of the view entertained by Professor Goodsir, that they are developed from a common structure.

He next described a dissection of the thymus and thyroid, which he had made in an adult male Hartebeest (*Bubalus Caama*). The thyroid in this animal was separated into two distinct lateral lobes, each lobe having connected with it a long slender glandular process, which passed down the sides and front of the trachea, behind the sternum, into the anterior mediastinum. These glandular processes exhibited microscopically the characters of the thymus, so that, both as regards structure and position, they must be regarded as constituting that gland.

The thymus and thyroid glands in the Nylghau (*Antilope picta*) were then described. The animal dissected was a magnificent specimen of an adult male, standing one foot above the recorded average height of the male. In it the thyroid was divided into two distinct lateral lobes, each lobe extending from the cricoid cartilage as far as the fourth tracheal ring. Situated on the anterior surface of the trachea, and on the crico-thyroid membrane between these thyroidean lobes, were scattered lobules of glandular tissue of a slightly reddish tint. These were not connected with the thyroid, but were lying in the cellular tissue between its lobes. Similar scattered lobules extended for some distance down the trachea, but about thirteen inches above the sternum they became aggregated together, so as to form two long lines of glandular tissue, which passed beneath the sternum into the anterior mediastinum. Structurally this gland presented the character of the thymus. It corresponded also to it in position. In addition to the proper gland structure, the microscope brought into view numerous three-sided prismatic crystals, resembling those of the triple phosphate, lying in and about the connective tissue of the gland.

From the evidence afforded by the dissection of the Hartebeest and Nylghau, the conclusion must be drawn that in these Antilopidæ the thymus is a permanent gland; for there could be no doubt but that both these animals had reached the adult period of life, and even acquired a considerable age,—their large size, and the worn appearance of the teeth rendered this sufficiently manifest. So far, then, as regards these animals, the thymus must be looked upon as possessing a more enduring function than has hitherto been ascribed to it in the economy,—not disappearing or altogether degenerating in the early period of extra-uterine life, but persisting even in the adult animal. The paper concluded by some remarks upon the thyroid and thymus glands in the human subject.

5. Notice on the Boring of the Pholadidæ. By Alexander Bryson, Esq., President of the Royal Physical Society.

In this communication the author referred to the various theories advanced to account for the boring of the Pholadidæ in rocks.

The first hypothesis, which supposes that the molluscs perforate by means of the rotation of the valves acting as augers, he disproved by exhibiting old individuals of the *Pholus crispata* with the dentated costæ as sharp as in any young specimen. That these animals bore by silicious particles secreted by the foot, as suggested by Mr Hancock, has been disproved by microscopic observation; and that currents of water set in motion by vibratile cilia, seemed also insufficient to account for the phenomenon.

Another theory supposes that an acid is secreted by the foot, capable of dissolving the rock. This the author showed was not tenable, as the strongest Nordhausen sulphuric acid fails to dissolve aluminous shales and Silurian slates; and also that any such acid secretion would act more readily on the valves themselves.

From many experiments on the cutting of hard silicious substances, the author found that the softer the substance was in which the cutting material was impacted, the greater the amount of the work done. He was thus led to the conclusion that the Pholadidæ bore with the strong muscular foot alone, and that they obtain the silica from the waves or the arenaceous rocks in which they are found; and hence there is no necessity for either an acid or silicious secretion. That the foot was the boring apparatus, and not the valves, he proved from a specimen of a Pholas hole in shale, where the pedal depression of the animal was distinctly seen.

He also exhibited a piece of glass bored to the depth of 1·50 of an inch, by means of the point of the finger and emery alone.

Tuesday, 17*th April* 1860.—THOMAS STEVENSON, Esq., Councillor, in the Chair.

The following Communications were read:—

1. On the Birds of Linlithgowshire. By Rev. John Duns, F.R.S.E., Torphichen.

The author, having referred to the value of carefully prepared reports on the ornithology of particular districts, characterised the physical features of that part of Linlithgowshire in which his observations had mainly been made, and showed that it is well fitted for the support of a comparatively large number of species of birds. He stated that the following list had been gradually formed as the result of observations spread over fifteen years. Most of the species named had come under his own notice; for a few he had been indebted to Thomas Durham Weir, Esq. of Boghead, an accurate observer. The classification followed is that which he had found most helpful to his own studies. Species seldom met with are printed in italics.

ORDER I.—RAPTORES.

Families.	Genera.	Species.
FALCONIDÆ.	Milvus	—*M. Regalis.*
	Falco	—*F. æsalon*, F. tinnunculus.
	Accipiter	—A. nisus.
	Circus	—C. cyaneus.
STRIGIDÆ.	Strix	—S. flammea, S. aluco, S. otus, *S. brachyotus.*

ORDER II.—SCANSORES.

CUCULIDÆ.	Cuculus	—C. canorus.

ORDER III.—PASSORES.

A. Fissirostres.

CAPRIMULGIDÆ.	Caprimulgus	—C. Europæus.
HIRUNDINIDÆ.	Hirundo	—H. rustica, H. urbica, H. riparia.
	Cypselus	—C. murarius.
HALCYONIDÆ.	Alcedo	—A. ispida.

B. Tenuirostres.

CERTHIADÆ.	Certhia	—C. familiaris.
	Troglodytes	—T. Europæus.

C. Dentirostres.

TURDIDÆ.	Turdus	—T. merula, T. torquatus, T. pilaris, T. viscivorus, T. musicus, T. iliacus.
	Cinclus	—C. Europæus.
AMPELIDÆ.	Bombycilla	—B. garrula.
LANIADÆ.	Lanius	—L. excubitor.
SYLVIADÆ.	Sylvia	—S. rubecula, S. hortensis, S. atricapilla, S. cinerea, S. phœnicurus, *S. locustella,* S. arundinacea, *S. phragmites.*
	Phillopneuste	—Ph. sylvicola, Ph. trochilus.
	Regulus	—R. auricapillus.
	Saxicola	—S. œnanthe, S. rubetra, S. rubicola.
	Accentor	—A. modularis.
	Parus	—P. cæruleus, P. major, P. longicaudatus, P. ater.
	Motacilla	—M. Yarrelli, M. boarula, M. flava (Budytes Rayi).
MUSCICAPIDÆ.	Muscicapa	—M. grisola, *M. luctuoso.*

D. Conirostres.

CORVIDÆ.	Corvus	—C. corone, C. cornix, C. frugilegus, C. monedula.
	Garrulus	—G. melanoleuca, G. glandarius.

Families.	Genera.	Species.
STURNIDÆ.	Sturnus	S. vulgarus.
	Pastor	*P. roseus.*
FRINGILLIDÆ.	Pyrgita	P. domestica.
	Fringilla	F. montifringilla, F. cœlebs, *F. spinus*, F. carduelis, F. cannabina, F. linaria, F. flavirostris, *F. borealis.*
	Loxia	L. chloris, L. pyrrhula, *L. Europœa*, *L. coccothraustes.*
	Emberiza	E. citrinella, E. miliaria, E. nivalis, E. schœniculus.
	Alauda	A. arvensis, *A. arborea.*
	Anthus	A. pratensis, *A. arboreus.*

ORDER IV.—COLUMBÆ.

Families.	Genera.	Species.
COLUMBIDÆ.	Columba	C. palumbus.

ORDER V.—GALLINÆ.

Families.	Genera.	Species.
TETRAONIDÆ.	Tetrao	T. Scoticus, *T. tetrix.*
	Perdix	P. cinerea, *P. coturnix.*
PHASIANIDÆ.	Phasianus	Ph. Colchicus.

ORDER VI.—GRALLATORES.

Families.	Genera.	Species.
ARDEADÆ.	Ardea	A. cinerea.
	Botaurus	*B. stellaris.*
CHARADRIADÆ.	Charadrius	C. hiaticula.
	Pluvialis	P. aurea, P. squatorala, *P. morinellus.*
	Vanellus	V. cristatus.
	Hæmatopus	H. ostralegus.
	Strepsilas	S. interpres.
	Calidris	C. arenaria.
SCOLOPACIDÆ.	Scolopax	S. rusticola, S. gallinago, S. gallinula.
	Numenius	N. arquata.
	Totanus	T. hypoleucus, T. calidris.
	Tringa	T. variabilis, *T. cinerea.*
RALLIDÆ.	Rallus	*R. aquaticus.*
	Crex	C. pratensis.
	Gallinula	G. chloropus.
	Fulica	F. atra.

ORDER VII.—PALMIPEDES.

Families.	Genera.	Species.
ANATIDÆ.	Anas	A. anser.
	Boschas	B. fera, B. *crecca.*
COLYMBIDÆ.	Podiceps	*P. minor*, *P. auritus.*
LARIDÆ.	Larus	L. ridibundus, L. marinus, L. canus, L. argentatus.
	Sterna	S. hirundo.

Marine Species named are to be met with on the shore of the Frith of Forth, between Grangemouth and Queensferry. In a series of Notes, the author described, at considerable length, the structure and habits of many of the Species named in the list, and referred to specimens on the table in illustration of his remarks.

2. On an unusual Drought in the Lake District in 1859. By John Davy, M.D., F.R.SS. Lond. & Edin.

This occurrence, following an unusual fall of rain in January, took place in May, June, and July. The ordinary amount of rain in these months is,—taking the average of the last eleven years,—at Lesketh How, Ambleside, 12·36 inches; during the months in question, at the same place, it was only 4·54 inches.

Three tables are given by the author in elucidation; the first relating to the fall of rain in five different places in the district; the second affording a summary of general meteorological observations at Kendal, more or less applicable to other parts of the district; the third containing, for the sake of comparison, the rain-fall at various places in the United Kingdom.

The author concludes with noticing the abnormal state of the weather during the whole of the year, marked by great vicissitudes of wet and drought, of heat and cold, and their effects, especially on vegetation.

3. On the Constitution of the Essential Oil of Cajeput. By Mr Maximilian Schmidl, Assistant to Dr T. Anderson, University of Glasgow.

The author shows, that oil of cajeput is a mixture of an oil boiling about 175° Cent., and one or more oils of higher boiling point. In the present paper he investigates the first of those substances. When purified by repeated distillation, it is a colourless, limpid fluid, which by analysis and determination of its vapour density, is shown to have the formula $C_{20}\ H_{16} + 2HO$. When treated with anhydrous phosphoric acid, it is decomposed, and yields a mixture of three different hydrocarbons, to which the author gives the names of Cajputene, Isocajputene, and Metacajputene. The two former, though differing in properties, have both the formula $C_{20}\ H_{16}$. The last, which is a very heavy oil, with a lemon yellow colour and brilliant fluorescence, is $C_{40}\ H_{32}$.

Monohydrate of Cajputene, $C_{20}\ H_{16} + HO$, is obtained by treating the original oil with commercial sulphuric acid at the boiling temperature, under particular precautions. The substance condenses to 4 volumes of vapour, although containing one atom of oxygen.

Hexhydrate of Cajputene, $C_{20}\ H_{16} + 6HO$ is obtained by agitating the bihydrate with dilute sulphuric acid, and leaving the mixture at rest for some time. Beautiful crystals gradually form in the mixture. The hexhydrate melts at 120° and solidifies at 85°. It is soluble in boiling alcohol and ether, from which it is deposited on cooling. Another compound, the constitution of which is not yet determined, is obtained by the action of dilute nitric acid in the cold.

Bihydrochlorate of Cajputene, $C_{20}\ H_{16} + 2HCl$ is obtained by mixing the oil with one-third of its bulk of strong aqueous hydrochloric acid, and then passing a current of the gas through the mixture; after the lapse of 10 or 12 minutes, the whole solidifies into a mass of crystals. These, when purified by expression and crystallisation from boiling alcohol, melt at 53°, and when repeatedly distilled, or when acted on by alcoholic potash, lose one-half of hydrochloric acid, and yield the monohydrochlorate. It is entirely devoid of taste and smell, and in this respect differs remarkably from the isomeric compound obtained from oil of turpentine.

Monohydrochlorate of Cajputene, $C_{20} H_{16} + HCl$ is an oily fluid, with a pleasant ethereal odour.

Tetrabromide of Cajputene, $C_{20} H_{16} Br_4$. Bromine is added to the bihydrate, and the mixture left for some weeks, when a granular substance is seen to deposit. As soon as this is observed, the whole is dissolved in boiling alcohol, and on cooling, glittering scales, resembling cholesterin, are deposited. It is soluble in alcohol and ether, melts at 60°, and may be distilled apparently unchanged.

Hydriodate of the hydrate of Cajputene. When iodine is added to oil of Cajeput, the temperature rises, and on cooling a black crystalline compound is deposited. This substance, after purification by cold alcohol, forms black crystals fusible at 80°, and very readily decomposed. Its formula is $C_{20} H_{16} HO + HI$.

Hydriodate of Cajputene, $C_{20} H_{16} + HI$. To obtain this compound, oil of cajeput is mixed with a solution of iodine in bisulphide of carbon, and to this a solution of phosphorus, in the same menstruum, is added. A brisk reaction takes place, the temperature rising to about 80°. After some weeks fine black crystals are deposited, which are soluble in alcohol and ether, and exceedingly stable, being unaffected even by alkaline solutions.

The author proposes to make the composition of the other constituents of the oil of cajeput the subject of a future paper.

4. On the Action of Chlorine on Citric Acid. Hexachlorinated Acetone. By John Galletly, Esq.

At the recommendation of Dr Anderson, Glasgow University, I have re-examined the oil which Plantamour obtained by acting on citric acid with chlorine. Owing to the slowness with which it is formed, its complete investigation is somewhat lengthened, and the following results are all I have as yet obtained. A considerable part of the work has been done in the Glasgow College Laboratory.

Plantamour found that when a strong solution of citric acid was exposed to the action of chlorine in the sunshine, a heavy oil appeared on the surface of the liquid, gathering in drops and sinking to the bottom. He describes the properties of this oil,* and ascribes to it the formula $C_8 Cl_8 O_3$. In his examination of acetone,†

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* See Rapport Annuel de Berzelius 7^{e} année.

† Nachricht, von der Gesellsch, der Wiss. Zu Göttingen. 1853. No. 9.

Staedeler found that a substitution product of this body, with five atoms of chlorine replacing hydrogen, resembled Plantamour's oil in every respect, and that the percentage composition of hexachlorinated acetone corresponded exactly with Plantamour's formula. There could therefore be very little doubt left as to its composition; but as I had the following analyses made before I was aware of Staedeler's suggestion, and as Plantamour procured a potash salt from it to which he assigns a formula having eight atoms carbon, and as his own analyses were not given in the memoir, I have published the following to remove any doubt.

Upwards of twenty half-gallon bottles filled with chlorine, having a little of a saturated solution of citric acid in each, required many weeks exposure to sunshine to yield about a fluid ounce of the oil. There seemed to be no other substance formed, unless, perhaps, hydrochloric acid and water. After drying over chloride of calcium it distilled entirely about 400° Fahr. I found its density at 60° Fahr. to be 1·748. Its properties agree very exactly with Plantamour's description.

I. { 7·200 grs. substance gave
3·620 grs. carbonic acid,
·200 grs. water.

II. { 9·250 grs. substance gave
29·790 grs. chloride of silver.

	Experiments. I.	Experiments. II.	Theory.		
Carbon,	13·71	—	13·58	C_6	36
Chlorine,	—	79·67	80·38	Cl_6	213
Oxygen,	—	—	6·04	O_2	16
			100·00		265

The following data were obtained in determining the vapour density:—

Temperature of air 20° Cent.,
,, of vapour at sealing 247° Cent.,
Excess of weight of balloon ·810 gramme,
Capacity of balloon, 165 cub. centimètres,
Residual air, . 7 ,, ,,
Barometer, 30·20 inches,
Density, 9·417 inches.

The formula $C_6\,Cl_6\,O_2$ requires $265 \times {\cdot}0346 = 9{\cdot}169$.

This oil is not decomposed by sodium, or at least very slowly, even with heat. It is not affected by boiling with oxide of silver, oxide of mercury, or even baryta water.

When the substance is agitated with water and cooled to 6° Cent. a crystalline hydrate is formed, which, according to Plantamour, fuses at 15° Cent. = 59° Fahr. This hydrate is $C_6 Cl_6 O_2 + 2HO$ (Staedeler). It often appears as a net-work of long crystals on the sides of the bottles in which the oil is formed, but these crystals may sometimes be heated considerably above the fusing point given by Plantamour without melting. I found, on keeping the oil under water for some months, that it turned into an opaque white mass of crystalline plates, which did not fuse till heated to a temperature of about 40° Fahr. When these crystals were melted again under water, they solidified shortly after cooling, without the fusing point being lowered. As this does not happen when the oil is freshly made, it is probable that the hexachlorinated acetone passes into some isomeric modification like the analogous body chloral. This hydrate disolves very readily in ether in the cold, giving long crystals covering the sides of the basin as the ether evaporates. If heat be employed to dissolve the crystals, the hydrate is decomposed and the oil separates.

When gaseous ammonia is passed into hexachlorinated acetone, it becomes solid, and there is an evolution of chloroform. The solid substance was washed with water, and crystalised from alcohol, when it formed large pearly square tables. A few grains of this body left some hours in the water-bath entirely volatilises. Its character and composition agree exactly with trichloracetamide, the following numbers having been obtained on analysis :—

	Theory.	Experiment.
Carbon	14·77	14·97
Hydrogen	1·23	1·52
Chlorine	65·54	65·14
Nitrogen	8·61	8·59
Oxygen	9·85	—
	100·00	

The same body is got by using aqueous ammonia; the decomposition taking place according to the following equation :—

$$C_6 Cl_6 O_2 + NH_3 = C_4 Cl_3 O_2 NH_2 + C_2 H Cl_3$$

Hexachlor. Acetone. Chloracetamide. Chloroform.

The products of decomposition with ammonia are therefore the same as those obtained when this alkali acts on the chlorinated ethers and aldehydes. I got a similar result with potash, which differs from the experience of Plantamour, who found the salt of a new acid which he names Bichloroxalic Acid. On dissolving the oil in alcoholic potass, it became warm and deposited chloride of potassium; and the alcoholic mother liquor, when left to spontaneous evaporation, gave long silky needles containing 53·08 per cent. chlorine. This agrees exactly with trichloracetate of potass, which contains 52·85 per cent. chlorine. The decomposition being evidently

$$\begin{array}{ccccccc} C_6\,Cl_6\,O_2 & + & KO\,HO & = & C_4\,Cl_3\,KO_4 & + & C_2\,H\,Cl_3 \\ \text{Hexachlor.} & & \text{Acetone.} & & \text{Trichloracetate Potas.} & & \text{Chloroform.} \end{array}$$

The paper in which these crystals were pressed when heated with water gave a solution which abundantly reduced nitrate of silver, showing the presence of formic acid from the further decomposition of the chloroform by the alcoholic potass.

It seemed to me probable that, by treating ethylate of soda with the oil, a decomposition would ensue in which a homologue of chloroform would be obtained, the ethyl replacing the hydrogen in this body. The decomposition, however, turned out differently. A considerable quantity of trichloracetic ether was produced, easily recognised by its fragrant peculiar odour, and among other bodies, chloroform and common salt.

The remainder of my material was expended in one or two preliminary experiments on its action on the volatile organic bases. It appears that like ammonia they give ordinary chloroform, when one of the radicals is hydrogen, the compound radical entering the chloracetamide. Thus the decomposition with aniline is as follows:—

$$\begin{array}{ccccccc} (C_{12}H_5)\,H_2N & + & C_6Cl_6O_2 & = & C_4Cl_3O_2NH\,(C_{12}H_5) & + & C_2\,H\,Cl_3 \\ \text{Aniline.} & & \text{Hexachlor Ac.} & & \text{Trichloracetanilide.} & & \text{Chloroform.} \end{array}$$

The change occurs in the cold. Trichloracetanilide crystalises in colourless prisms of some size. It is insoluble, or very sparingly soluble, in cold water, but dissolves in warm water pretty readily, filling the liquid with silky needles as the solution cools. I expected that, like chloracetamide, it would sublime without decomposition, which it partly did; but there is a great loss, and much charcoal is left. I had barely enough left to ascertain by experiment its exact composition, and the analysis of the sublimed substance gave an excess both in carbon and hydrogen, but the method of formation and its properties leave no doubt about the formula. As I intend to continue the subject, I shall probably have an opportunity of publishing an exact analysis. When rapidly distilled with strong solution of potass, or with soda lime, there is formed besides aniline a new volatile base, with a very peculiar pungent odour. This body seems rather easily decomposed, but I had far too little to be able to give a particular account of it at present.

When the oil is mixed with a base containing a triatomic radical, such as lutidine, no chloroform is evolved. The liquids mix, but there is no apparent action in the cold, and when heat is applied, the decomposition is so violent that only a charcoal-like residue is left.

The summer being the only season when the oil can be conveniently prepared by Plantmour's process, I have made some experiments with a view to find other methods of procuring it, which I may mention here. Staedeler found that chlorinated acetones were procured by distilling citric acid with chlorate of potass and hydrochloric acid. The oil which I procured in this way had a specific gravity of 1·726, and boiled between 360° and 400° F. It was evidently a mixture of his pentachlorinated and hexachlorinated acetone. It is extremely irritating, causing great pain in the eyes, and can scarcely be distilled except with special arrangement for carrying off the vapours. I procured by a few distillations a portion of fluid having very nearly the specific gravity and boiling point of Plantamour's oil, but the irritating action of its vapour was scarcely diminished. It gave likewise the solid compound when mixed with aniline, so that there is no doubt about the identity of the bodies. I distilled in the sunshine the mixture given above, in the hope that the product might be only hexachlorinated acetone, but a violent explosion put an end to the experiment. The mixture obtained in this way is evidently the same as that Plantamour got by acting with chlorine on a citrate.

The similar oil got from citric acid, oxide of manganese, and hydrochloric acid, had only a specific gravity of 1·50 and a portion boils even below 212°, but the greater part seems to be pentachlorinated acetone. From the low boiling point of some of the fractions, it may be questioned whether these bodies are all chlorinated acetones.

The action of chlorochronic acid is too violent, and I have not been able to find a better mode of preparing the oil than the slow action of chlorine in the sunshine, which gives it pure without any trouble.

The pentachlorinated acetone decomposes with aniline in a similar manner with the body described, giving what is probably bichloracetanilide.

5. Notice of a Panoramic Sketch of Kashmir, recently received from India. By Professor C. Piazzi Smyth.

Monday, 30th April 1860.—Professor CHRISTISON, V. P., in the Chair.

The following Communications were read:—

1. Account of the Asafœtida Plants (*Narthex Asafœtida* (Falconer), which have recently flowered and fruited in the Edinburgh Botanic Garden. By Professor Balfour.

The author gave an account of the cultivation of plants of Asafœtida in the Edinburgh Botanic Garden since the year 1842, and of the flowering and fruiting of specimens in 1858 and 1859. He then described the characters of the plant (*Narthex Asafœtida* of

Falconer), and illustrated his communication by specimens of the plant, by photographs, and by drawings from the accurate pencil of Dr Greville.

2. On the Composition of the Glassy Surface of some Vitrified Forts. By Thomas Bloxam, F.C.S., Assistant Chemist to the Industrial Museum of Scotland.

3. On the Reduction of Observations of Underground Temperature, with applications to Professor Forbes' Edinburgh Observations and the continued Calton Hill Series. By Professor William Thomson, Glasgow.

The principle followed in the reductions which form the subject of this communication may be briefly stated thus:—

The varying temperature during a year, shown by any one of the underground thermometers on an average for a series of years, is expressed by the ordinary method in a trigonometrical series of terms representing simple harmonic variations*,—the first having a year for its period, the second a half year, the third a third part of a year, and so on. The yearly term of the series is dealt with separately for the thermometers at the different depths, the half yearly term also separately, and so on, each term being treated as if the simple periodic variation which it represents were the sole variation experienced. The elements into which the whole variation is thus analysed are examined so as to test their agreement with the elementary formulæ by which Fourier expressed the periodic variations of temperature in a bar protected from lateral conduction, and experiencing a simple harmonic variation of temperature at one end, or in an infinite solid experiencing at every point of an infinite plane through it a variation of temperature according to the same elementary law. In any locality in which the surface of the earth is sensibly plane and uniform all round to distances amounting at least to considerable multiples of the depth of the lowest thermometer, and in which the conducting power of the soil or rock below the surface is perfectly uniform to like distances round and below the thermometers, this theory must necessarily be found in excessively close agreement with the observed results. The comparison which is made in the investigations now brought forward must be regarded, therefore, not as a test of the correctness of a theory which has mathematical certainty, but as a means of finding how much the law of propagation of heat into the soil is affected by the very notable deviations from the assumed conditions of uniformity as to surface, or by possible inequalities of underground conductivity existing in the localities of observation. When those conditions of uniformity are perfectly fulfilled both by the surface and by the substance below it, the law of variation in the interior produced by a simple harmonic variation of temperature at the surface, as investigated by Fourier, may be stated in general terms in the three following propositions:—(1.) The temperature at every interior point varies according to the simple harmonic law, in a period retarded by an equal interval of time, and with an amplitude diminished in one and the same proportion, for all equal additions of depth. (2.) The absolute measure in ratio of arc to radius, for the retardation of phase, is equal to the diminution of the Napierian logarithm of the amplitude; and each of these, reckoned per unit of length as to augmentation of distance from the surface, is equal to the square root of the quotient obtained by dividing the product of the ratio of the circumference of a circle to its diameter into the thermal capacity of a unit of bulk of the solid, by the thermal conductivity of the same estimated for the period of the variation as unity of time. (3) For different periods, the retardations of phase, measured each in terms of a whole period, and the diminutions of the logarithm of the amplitude, all reckoned per unit of depth, are inversely proportional to the square roots of the periods.

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* By a simple harmonic variation is meant a variation in proportion to the height of a point which moves uniformly in a vertical circle.

The first series of observations examined by the method thus described were those instituted by Professor Forbes, and conducted under his superintendence during five years, in three localities of Edinburgh and the immediate neighbourhood; (1.) The trap rock of Calton Hill; (2.) The sand below the soil of the Experimental Gardens; and (3.) The sandstone of Craigleith Quarry. In each place there were, besides a surface thermometer, four thermometers at the depths of 3, 6, 12, and 24 French feet respectively. The diminution in the amplitude, and the retardation of phase in going downwards, has been determined for the annual, for the half-yearly, third-yearly, and the quarterly term, on the average for these five years for each locality. The same has been determined for the average of twelve years of observation, continued on Calton Hill by the staff of the Royal Edinburgh Observatory.

The following results with reference to the annual harmonic term are selected for example (see Table, bottom of p. 135):—

If Fourier's conditions of uniformity, stated above, were fulfilled strictly, the numbers shown in the second column for each locality would be equal to one another, and equal to those in the third column. The differences between the actual numbers are surprisingly small, but are so consistent that they cannot be attributed to errors of observation. It is possible they may be due to a want of perfect agreement in the values of a degree on the different thermometric scales; but it seems more probable that they represent true discrepancies from theory, and are therefore excessively interesting, and possibly of high importance with a view to estimating the effects of inequalities of surface and of interior conductivity. The final means of the numbers in the second and third columns are

Calton Hill	·11702
Experimental Garden	·11061
Craigleith Quarry	·06988

The thermal capacities of specimens of the trap rock, the sand, and the sandstone of the three localities were, at the request of Professor Forbes, measured by Regnault and found to be respectively

·5283, ·3006, and ·4623.

Hence, according to proposition (3), stated above, the thermal conductivities are as follows:—

Trap rock of Calton Hill,	121·2
Sand of Experimental Garden,	77·19
Sandstone of Craigleith Quarry,	273·6

These numbers do not differ much from those given by Professor Forbes, who for the first time derived determinations of thermal

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Average of five years, 1837 *to* 1842.

	Retardation of phase in days, per French foot of descent.	Retardation of phase in circular measure, per French foot of descent,	Diminution of Napierian logarithm of amplitude, per French foot of descent.
Calton Hill.			
3 feet to 6 feet.	......	·11635	·12625
6 „ 12 „	......	·11344	·12156
12 „ 24 „	......	·11490	·10959
Mean, or 3 to 24.	6·68 days.	·1147	.1154
Experim^l. Garden.			
3 feet to 6 feet.	......	·11635	·10037
6 „ 12 „	......	·11929	·11304
12 „ 24 „	......	·10617	·10844
Mean, or 3 to 24.	6·6 days.	·11137	·10859
Craigleith Quarry.			
3 feet to 6 feet.	......	·063995	·09372
6 „ 12 „	......	·066903	·06304
12 „ 24 „	......	·066903	·06476
Mean, or 3 to 24.	3·86 days.	·066489	·06840

conductivity in absolute measure from observations of terrestrial temperature. In consequence of the peculiar mode of reduction followed in the present investigation, it may be assumed that the estimates of conductivity now given are closer approximations to the truth. To reduce to the English foot as unit of length, we must multiply by the square of 1·06575; to reduce, further, to the quantity of heat required to raise 1 lb of water by 1° as unit of heat, we must multiply by 66·447; and lastly, to reduce to a day as unit of time, we must divide by 365¼. We thus find the following results:—

Trap rock of Calton Hill, . . .	23·5
Sand of Experimental Garden, . .	15·0
Sandstone of Craigleith Quarry, . .	53·5

These numbers show the quantities of heat per square foot conducted in a day through a layer of the material one foot thick, kept with its two surfaces at a difference of temperature of one degree,—the unit of heat being, for instance, the quantity required to raise 1000 lbs of water by $\frac{1}{1000}$th of a degree in temperature.

The same system of reduction applied to the observations continued at the Calton Hill station, has led to results from which the following are selected:—

Average annual term for 12 years—1842 to 1854—Trap rock to Calton Hill.

	Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.
Depths below surface in French feet.	Proportionate Diminution of amplitude.	Diminution of Napierian logarithm of amplitude per French foot of descent.	Retardation of epoch in circular measure, per Fr. foot of descent.	Retardation of epoch in decimal of a year, per Fr. foot of descent.	Retardation of epoch in days, per Fr. foot of descent.
3 feet to 6 feet	·675	·1310	·1233	......	......
6 „ 12 „	·498	·1163	·1142	......	......
12 „ 24 „	·260	·1121	·1145	......	......
3 „ 24 „	·0875	·1160	·1157	·01841	6·724

By these results it will be seen that the discrepancies from the theory based on the hypothetical conditions of uniformity, noticed above as found in the reduction of the first five years' series of observations, are maintained with the same character, and to nearly the same amount, in the succeeding series of thirteen years. An investigation of the changes of conductivity and specific heat, which, if the ground were level and the surface uniform, would be required to account for these discrepancies, is made, so far as the data suffice for determining them. The paper concludes with the solution of some practical problems regarding the conduction of heat through rock possessing the conductivity determined by the reductions which form the chief part of the paper.

4. On a Method of Reducing Observations of Underground Temperatures, with its Application to the Monthly Means recorded in the Report of the Royal Observatory of Edinburgh, &c. By Professor Everett.

In this paper the same general method of reduction as that of Professor Thomson, explained in the preceding paper, is followed. The numerical labour is, however, much diminished by using the monthly means given in the observatory report as data for twelve equations of condition, instead of the methods by which Professor Forbes had obtained data for twelve, and Professor Thomson for thirty-two equations of condition. The method adopted in the present communication, although not susceptible of such minute accuracy as the more elaborate methods referred to, seems to be as accurate as is necessary for a fair representation of the phenomena, and has a great advantage in point of simplicity and ease of working. In the present communication the practical methods of calculating the amplitudes and arguments of the successive terms in the harmonic expression of a periodic variation are fully explained, as it is believed the method will be found useful in the reduction of almost every class of meteorological observations, and as in this country, at all events, there is not much familiarity with it among practical meteorologists.

Royal Physical Society.

Wednesday, 23d November.—Andrew Murray, Esq., President, in the Chair.

The following Communications were read:—

I. *Opening Address by the President*, Andrew Murray, Esq.

Mr Murray, after referring to the death of Professor George Wilson, and making some remarks on the nature of introductory addresses, proceeded to give an account of the progress of Entomology.—Beginning with the works on systematic entomology published during the last three years or so, *facile princeps*, whether in extent and importance of subject, or in the mode in which it is being executed, stands Lacordaire's *Histoire des Genres des Coleoptères*,—a work now in course of being published as one of the *Suites à Buffon*. For a long series of years, ever since the days of Fabricius, Latreille, Olivier, &c., when the whole number of insects known did not exceed many hundreds, down to the present day, when 100,000 species but faintly represent the number actually known (there are 90,000 species of insects of all orders in the Berlin Museum), entomology has been going on constantly increasing, without any systematic work or general treatise upon Coleoptera having been executed. An enormous number of species have been separately described in transactions and periodicals; numerous combined descriptions of new species peculiar to individual districts have been published; and also a number of local faunas, such as those of Erichson, Stephens, Redtenbacher, Mulsant, Heer, Rossi, &c. Many most valuable monographs and treatises upon special groups of Coleoptera had also been executed, such as Dejean's *Carabidæ*, Aubé's *Hydrocantharidæ*, Erichson's *Staphylinidæ*, Burmeister's *Lamellicornes*, Schonherr's *Curculionidæ*, and a host of others. To attempt to lick these into shape—to throw them all into one common systematic treatise, embodying at their proper places the thousands of independent notices scattered through a crowd of transactions and periodicals—disposing of questions of disputed synonymy—deciding the great questions of disputed arrangement, and where these appear wrong correcting them, or offering a new solution of the difficulty,—is a task requiring the patience of Sisyphus and the powers of labour of Hercules, combined with talents, attainments, and facilities possessed by very few. Such is the task which is now being successfully carried out by Professor Lacordaire. Five volumes have already appeared: the first is occupied with the *Carabidæ;* the second, the *Staphylinidæ* and *Clavicornes;* the third, the *Lamellicornes;* the fourth, the *Buprestidæ*, *Elateridæ*, and *Malacodermata;* and the fifth, which is only newly out, with the *Heteromera*. The manner in which a systematic view of the subject is given is this:—The characters of the different groups, larger and smaller, are separately detailed; a full exposition of the characters of each genus, with its synonymy, is then separately given; a notice of its geographical distribution, or any specialty relating to it, is added; and in a note, a list of all the species hitherto described is given in the shortest space possible, with occasional synonymical corrections. A beautiful atlas or volume of plates, giving figures and details of the rarer genera, is to accompany the work; and here, as in the text, every care is taken to save unnecessary expense. In the lists of the species given in the notes, for instance, instead of burdening them with the species which have been described in the chief monographs on the subject, Professor Lacordaire assumes that these are already in the library of every entomologist, and merely says, "To the 150 species (or whatever the number may be) described by Dejean, add the following." So in the plates, nothing that is to be found in works easily accessible is here repeated. The result is, that this is undoubtedly the most useful entomological work of the present day. It is a perfect storehouse of information, and forms a new starting-point from which entomologists may take a fresh departure. In according to it so much commendation, I am far from implying that it is perfect, or implicitly to be trusted to. It is *ex necessitate* in great part a compilation, and, as in all compilations, the accuracy of the work depends upon the accuracy of the original describer, not of the compiler. No doubt the correctness of many of the descriptions (particularly those in the first volume) had been tested and often improved by the author, but latterly less frequently so.

Next in importance to this work of Professor Lacordaire we have a number of very valuable monographs. The concurrent opinion of entomologists has been of late years expressed so strongly against the practice, once common, of giving isolated specific descriptions of individual insects in transactions or periodicals, that such descriptions are now becoming proscribed, unless where some special reason exists for signalizing an individual—as, for instance, its being of a very anomalous character, so that its true position may be matter of doubt, or its supplying a vacant gap, or furnishing an interesting unknown representative in one country of a group peculiar to another country. Except under such circumstances, one does not now often meet those isolated descriptions with which the young beginner used to essay his flight. Something connected is now looked for, and entomological writings now either assume the form of monographs or local faunas. I shall first glance at the recent monographs. One very important one is a monograph of the *Elateridæ* by M. Candeze of Liege (a pupil of M. Lacordaire), which has been executed with a care and skill worthy of his great master. M. l'Abbé de Marseuil's monograph of the *Histeridæ*, lately published in the *Annales* of the Entomological Society of France, is another most admirable specimen of what a monograph should be. In addition to a good description, an engraved outline of every species is given—an assistance which those who have puzzled over the great number of species apparently alike, and only distinguished by their delicate sculpturing and punctuation, will know how to appreciate.

Another important monograph has been brought to a close within the last three years—I mean Boheman's *Monographia Cassididarum*. The first volume has been before the public for some years; the second is now also, and the favourable verdict which had been pronounced upon the first is confirmed on the second. So many additional species have since been discovered, that he informs me that a supplement has become necessary, and will be published.

Two other monographs have appeared or been commenced upon other families of the subpentamerous *Phytophagiæ*. One a monograph upon the African *Cryptocephali*, published by M. Suffrian in the *Linnea Entomologica* two years ago. M. Suffrian had previously monographed the *Cryptocephalidæ* of Europe, and also those of North America. He is now engaged upon those of Australia, to which, although Mr Wilson Saunders' papers in the 4th volume of the *Transactions of the Entomological Society of London* formed a valuable contribution, much still remained to be done.

The other monograph to which I referred as lately published, in this group of families, is the Catalogue of the *Hispidæ* in the British Museum, by Dr Baly—1st volume. In accordance with the enlightened course followed by Dr Gray in publishing lists of the contents of the British Museum, these, from originally consisting of a mere list of what was in the museum, have gone on improving, first into a list both of described species already in the museum and of the desiderata not in the museum; then advancing into a description of new species in the museum besides those already described; and at last assuming the form of perfect monographs by the first authorities, containing their newest views of arrangement, the descriptions of species both old and new, and this not merely the new in the museum, but all the new that can be collected from every quarter; so that, while practically the interest of the museum in it is limited to the letters B M appended to the species it possesses, its real interest extends to the whole scientific world. Dr Baly's work is the production of a careful and acute naturalist, and is a credit to the science of this country.

Herr Gerstacker has commenced a work under the title of *Entomographica*, of which the first volume, lately published, contains a careful monograph of the *Endomychidæ;* and as M. Guerin-Meneville has also given his views on that family, first in Mr Thomson's *Archives Entomologiques*, and afterwards in his *Revue de Zoologie*, we may approach the study of that difficult family without fear of suffering from insufficient advice. I confess, however, that it is rather an *embarras de richesses*. It is possible to have too much of a good thing; and two monographs on the same subject, published by two eminent men at the same time, is a case in point. With the poet we may say—"How happy could I be with either, were t'other dear charmer away." We have seen the same thing happen before. It happened with

Kirby and Latreille. When Kirby brought out his chiefest work, the *Monographia Apum Angliæ*, Latreille's *Genera* appeared within a few weeks of it; but there the competition only shed lustre upon both. Working upon a nearly new subject without communication with each other, they hit upon the same divisions, established families and genera upon the same characters, and generally the results to which they came were so identical, as to give confidence to men of science that the subject had been carefully and conscientiously investigated, and that the conclusions to which they had come were to be relied upon. One fortunate accidental circumstance in that case prevented the embroilment of synonymy becoming so great as it would otherwise have been: Kirby, instead of giving separate generic or subgeneric names to his minor sections, indicated them merely by a greater or less number of asterisks or Greek letters, so that the generic names which had been given them by Latreille stood and answered both for his own sections and for those of Kirby. In this instance the same coincidence does not occur. Gerstacker has taken his generic characters chiefly from the parts of the mouth; Guerin-Meneville from the elytra, the legs, and the prosternum—which latter, however, has also been made use of by Gerstacker; but fortunately, before Gerstacker's work issued from the press, he had the opportunity of seeing Guerin's papers, and he points out the differences between them in an appendix.

Some other valuable monographs have appeared in various periodicals, or in occasional works which are issued by one or two zealous entomologists—in the *Opuscules* of Mulsant, the *Archives* of Thomson of Paris, the *Meletemata* of Kolenati, and the *Etudes Entomologiques* of Motschoulsky, &c. &c. In his *Opuscules*, Mulsant has published several valuable monographical papers, contributions to the history of the *Pedinites*, and others of the *Heteromera*. M. Motschoulsky has given a monograph of the *Lampyridæ* or glowworms, and of the *Malacodermata*, in his *Etudes*. Mr Thomson is publishing a monograph of the *Cicindelidæ*, with coloured figures of every species, in a style of "*luxe*" (we have no word to express the meaning) hitherto unequalled. He has also, with equal success, in his *Archives*, monographed the *Anacoli*, *Tragocephali*, &c., small but beautiful tribes of *Longicornes*. Several lesser monographs have also lately appeared, such as that of M. Bouldieu on the *Ptini* (*Ann. Soc. Ent. Fr.*); Du Bonvouloir, a separate work, on the *Throscidæ*, &c.

Let us now turn to the special additions to our knowledge of insects and of their geographical distribution. We naturally turn our eyes first to our native country, and inquire what advance British Entomology has been making? From various causes, the zeal of our entomologists has of late chiefly taken the direction of the *Lepidoptera*. Mr Stainton, by his personal energy, as well as by his work on the *Tineina*, has given a great impulse to the study of the *Microlepidoptera*; and his *Entomologist's Annual*, which has been the chief outlet to the votaries of British Entomology, has naturally been principally occupied with the department which its editor most affects. Mr Jansen, however, in that annual has done good service to the *Coleoptera*, by placing upon record the capture of such species as were not previously known to be British—or rather, not recorded in Stephens' *Manual* as British. This record of additions, which is prepared with much care, comes at a most suitable time to aid Mr Waterhouse in his unpretending but most laborious construction of a catalogue of all the British species of *Coleoptera*. As this is not merely a catalogue of all Stephens' species, with the addition of such novelties as Mr Jansen and others may have recorded, but a careful examination of every specimen in Stephens' collection, and an unravelling and compari- with continental names of his and Kirby's synonymes, so as to give us a true list of what is really native (no doubtful species being admitted), it is obvious that it is a work which could only be undertaken by a consummate entomologist. He has published about the half of it; and as, for the sake of certainty and harmony with the continental nomenclature in such difficult families as the *Staphylinidæ* and *Nitidulidæ*, he has expressly visited the continental collections, so as to see the true types of Erichson and others, we have a work of European value. I can truly say, that there is no work falling within my present subject which comprises so much skilled labour in such small space. This catalogue is the first step towards our procuring a good manual or entomological work in the English language, giving descriptions of the species found in Britain. Members are aware that we have no such work, and that at present, for good working books of reference, we must have recourse to the Continent, although even there they are not far ahead of us, no good work on the subject having been yet completed, although some are in progress. The British works which we do possess are Curtis's *British Entomology*, Wilson and Duncan's *Entomologia Edinensis*, and Stephens' *Illustrations* and *Manual*. Of these, Curtis's *British Entomology* gives a valuable series of coloured plates, which are so judiciously selected as to represent almost every one of our modern genera. The text is certainly not equal to the plates, and I do not think that Mr Curtis would now so consider it himself. Entomological descriptions are very different things from what they were when he wrote that work. The arrangement also is confused, owing to the plan adopted in bringing out the work (which was to give a little of each department at the same time, or nearly so), and the work generally would require to be revised to suit the modern state of the science. The plates, however, are unsurpassed: they have been published separately, and other use will yet be made of them. In the meantime they give what is equivalent to the genera of most of our British insects, and that not only in the *Coleoptera*, but in all the other orders. The book, however, is out of print, and, besides, was so expensive as to put it beyond general reach. There is now an equivalent to this book publishing in Paris, so far as the *Coleoptera* are concerned. It is styled the *Genera des Coleoptères d'Europe*, by Jacquelin du Val, a very accurate entomologist, who first distinguished himself, some years ago, by publishing a revision of that most perplexing family, the *Bembidia* of Europe. His present work gives a beautiful coloured figure, with details, of every genus known in Europe. The other English works capable of assisting him in this object are the *Entomologia Edinensis*, by the late Mr James Wilson and Mr W. Duncan, applicable only to a small portion of British species, and, moreover, out of print. Stephens' *Illustrations* and *Manual*, although of the highest value when they first appeared, are now felt to be so cumbered with blunders and inaccuracies, that many think it better not to use them at all.

A few more monographs of British species of certain groups, which are everything that one could wish, so far as they go, but only embrace a small part of the subject, will not supply the deficiency. I refer to Dawson's *British Geodephaga*, Walton's *British Curculionidæ*, Wollaston's *Atomariæ*. If I had not praised them, I would have added my own *Cercyon*, *Catops*, &c. By getting British entomologists to take up special groups in this way, we may at last obtain materials sufficient to make it easy for a compiler to throw the whole into one work. Until that is done, we must look abroad; and although we do not obtain even there anything complete which I could recommend, we shall there find what we want in progress, either in French or German, as our inclinations may lead us. On the one hand, we have Fairmaire and Laboulbène's *Faune Française*, of which the first volume only is published—a large duodecimo at a cheap price; on the other hand we have the *Insecten Deutschland*, which was commenced by Erichson, resumed, in a spirit and with an ability worthy of Erichson himself, by M. Schaum, M. Kraatz, and M. Kiesenwetter. M. Schaum has taken the *Geodephaga* and *Hydradephaga* in hand; M. Kraatz has supplied the *Staphylinidæ;* M. Kiesenwetter is engaged upon the *Buprestidæ* and *Elateridæ*. This is the work which I would recommend for British entomologists. The fauna of Germany is in many respects so similar to our own, that any new discoveries in Britain may be reasonably expected to be species already known there; and we thus have not only a most careful guide to the species already known in Europe, but the means of deciphering any novelties.

There is another book of a similar nature which would be very useful to British entomologists, but is in a manner superseded by the two preceding works—a series of volumes, by M. Mulsant, descriptive of different sections of Coleoptera in France. It is a good many years since he published the Lamellicornes of France, the Palpicornes of France, the Longicornes of France, the Securipalpes of France, and now we have the Heteromera of France. Every one must admit the value of M. Mulsant's works; but the extreme minuteness of detail renders them less popular than they deserve. So far as regards the European fauna, a great many additions can scarcely be expected. A number of new species from the Landes and the Pyrenees have been described by M. Kiesenwetter and others; and some interesting small species, constituting new genera, have been discovered by M. Jac. du Val, near Montpelier, and described in his *Genera des Coleopteres*. Spain, slow and lazy Spain, has done a little by the hands of Professor Graells of Madrid, and Dr Rosenham of Bonn has described some new species from Andalusia; but the chief novelties of interest have been drawn from two sources not thought of till of late years—namely, ants' nests and subterranean grottos. The additions drawn from the former source have been chiefly Staphylinidæ, and will be found in a paper by M. Kraatz upon the Termitophila (both those in the nests of termites and ants) published in the *Linnea Entomologica* last year. The Troglodytes or subterranean families have produced several interesting new eyeless species, and one or two genera. The most interesting points are the fact, that every new cave, or cave-district, produces not the old previously known cave-animals, but new species.

A curious blind new genus, *Leptomastax hypogeus*, has been found on the sands of the Bay of Becka, near Constantinople, and described first by M. Pirazzoli, and afterwards by M. Leon Fairmaire. It is peculiarly formed, allied to the Scydmænidæ, and has no affinity to any of the cave-insects we have yet seen, but has considerable resemblance to a small ant; and although found at large, as it were, I have no doubt it is an ant's nest species, and will yet be found in its proper residence.

Few new additions have been made to the fauna of the north of Europe. Prince Napoleon's expedition appears only to have produced one new species, described by Reiche under the name of *Patrobus Napoleonis;* but a good deal of useful geographical material relating to that quarter will be found in two papers by Maklin and Osten-Sacken, published in 1857 in the *Stettiner Ent. Zeitung*, which continues to go on prospering, and I trust long to prosper, under the able headship of its perpetual president Herr Dohrn (one of our foreign members), and nowise injured by the rise of its newly established formidable rival the *Berliner Entomol. Zeitschrift*, in which are to be found some very valuable papers. Among the more important original papers which have appeared in this Journal falls here to be mentioned a fauna of the Coleoptera of Greece, by Dr Kraatz and M. Kiesenwetter.

Russian and Siberian species have been described by Count Motschoulsky in his *Etudes Entomologiques*, and the *Bulletin of Moscow*. A considerable number of species taken during the Crimean war, in the Dobrudska, Crimea, and other shores of the Black Sea, have been described in the *Ann. de la Soc. Ent.* and elsewhere; and the Baron Chaudoir, as well as Count Motschoulsky, continue to enlarge our knowledge of the Caucasian and Mingrelian regions; and M. Kolenati, in his *Meletemata*, has gone systematically over this district.

Passing round into Africa by the shores of the Mediterranean, we have received from Messrs Reiche and Saulcy a considerable addition to our knowledge of the entomology of Palestine, a district of which we knew comparatively little. The materials from which this contribution has been derived were collected during the expedition by M. de Saulcy, who, the the Society may recollect, published an interesting account of his visit to the Dead Sea a few years ago.

Continuing our course round the Mediterranean, we find in Count Motschoulsky's *Etudes Ent.* the description of a number of the minuter Egyptian species overlooked by previous explorers. M. de Motschoulsky has extended his travels into a good many districts both in the Old and New World; and, as he is an assiduous collector as well as a rapid describer, we have a good many species from his pen from all parts of the world. He has for the last eight or nine years published every year a periodical, called his *Etudes Entomologiques*, in which much valuable material is to be found.

Continuing along the north coast of Africa, a number of new species have been described by Lucas from Algeria; but this applies less to the Coleoptera than to some of the other orders. Madeira should come in here. Mr Wollaston has published in the form of a catalogue, consisting of a complete series of the species of Madeiran Coleoptera in the British Museum, presented by him to that institution, a number of new species detected since the publication of his great work on the *Insecta Maderensia*. He returned last summer; with an immense amount of materials for a fauna of the Canary Isles; but I grieve to say, that his state of health has been too bad to allow of his putting them into shape.

Little new has been recorded on the west coast of Africa until we come to Old Calabar. My descriptions of new species from that district, which by a sort of legal fiction have been read here, have gone on appearing in the *Annals of Natural History* from time to time, as suited my own convenience. M. Chevrolat, to whom I confided the Longicornes, published a century of new species, among which are some of singular beauty. Dr Baly has also described some of the new genera of *Phylophages* in the *Annals of Natural History*.

Stimulated, I firmly believe, by the number of novelties sent home by our friends in Old Calabar, and distributed among continental entomologists, two eminent entomologists resident in Paris, Count Murzscheck and Mr James Thomson, organised an expedition to the neighbouring territory of Gabou, and sent out M. Herz Deyrolle, son of the highly esteemed dealer in Paris, on an expedition to that country. The results of his collecting, at least those species which are new, are described in the second volume of Mr Thomson's *Archives*. From his descriptions it appears that a considerable number of our Old Calabar species are found in Gabou, but that a large proportion also is distinct; and looking to the relative number of species found, I should say that our unpractised amateurs need not hide their heads in point of collecting with this crack collector of Paris.

To the south lies the kingdom of Angola, of whose coleopteral fauna little more is known than what is contained in Erichson's fauna of that district, published in *Wiegner Archiv.* in 1843.

The next zoological district which meets us as we journey round Africa is the Cape, and we may include under that head the whole coast from the Cape itself north to Natal, both inclusive. A good fauna of the Cape was greatly wanted. Several authors, such as Klug, had described a certain portion of its Coleoptera; and other authors, such as Burmeister and Schonherr, in their great works on the *Lammellicornes* and *Curculionidæ*, have of course described many which fell within the scope of their subject; but there was no general systematic work until Boheman, four or five years ago, commenced his *Insecta Caffraria.* The first volume was then published; and the second, containing the Lamellicornes, has appeared during the last year. Like all Boheman's works, this is most satisfactory. I find a general impression among working entomologists, that Boheman's descriptions come nearer to what an entomological description should be, not only in acuteness and discrimination, but also in attaining the proper medium, in point of length, than those of any other living entomologist.

Madagascar stands blank. Nothing has been done to it during the period I am reviewing, either in the way of collecting or describing, unless we reckon as something the unsuccessful attempt of Madame Pfeiffer. The cruelty and atrocity of the queen have made it forbidden ground, and it is like to continue so, at all events, during her life, unless some fortunate political squabble with some of our ships shall rouse our Government to interference, or a lack of other openings for martyrdom shall tempt some of our missionaries to that wondrous land.

Signor Bertolini has supplied the collections of Europe with a fair sample of the productions of Mozambique, and has also given descriptions of the most striking.

Passing Abyssinia, the Red Sea, Arabia, Persia, and the Himalayas, which have produced little or nothing new during the last few years, we shall find that a good deal has been done in India, more especially in its southern portion and Ceylon. M. Nietner has discovered and described a good many new species from Ceylon, particularly a number of minute species generally overlooked. Count Motschoulsky, in his last two years' *Etudes Entomologiques*, has also described a great many minute species, some of them of very singular and abnormal appearance. A very considerable number of species of Coleoptera from Ceylon have also been described in the *Annals of Natural History*, during the last year, by Mr Francis Walker, well known for his papers on the Chalcidites; for his two volumes on British Diptera (*Insecta Britannica*); and for his Catalogues of Moths in the British Museum. Unfortunately, Mr Walker has sacrificed everything to shortness, so that, without their apology, his descriptions, so far as regards decipherableness, must rank with those of Linneus and Fabricius. Nagpore has its name, which was already distinguished in natural science, rendered still more so during the last few years through the exertions of two Scottish missionaries, our friends Mr Hislop and Mr Hunter. It is chiefly to geology that their studies have been directed, and they have brought home with them a number of fossil parts of insects, which, through their kindness, were placed in my hands for examination and description. These, so far as they were in a condition to allow of its being done, I described in a paper which formed part of Mr Hislop's general geological work, now in course of publication by the Geological Society of London. The materials were too few and imperfect to generalize from; but they all belonged to the great families still common on the Indian continent. *Buprestidæ*, *Curculionidæ*, &c. The structure, however, of some of them seemed to have more affinity with certain modern Australian forms. This, however, rests on a mere hazy resemblance, insufficient to warrant reliable deductions. The arrivals of species from China, Hongkong, Singapore, Java, &c., have been few of late years, and the descriptions of new species still fewer, being limited to a few isolated species, described for their beauty, such as *Carabus Fiduciarus*, *Carabus Celestis*, &c., and a few Phylophaga, occasionally described by Mr Baly.

M. Motschoulsky, however, has given us the descriptions of a number of species picked up at the mouth of the Amoor and in Japan by M. Gaskevitch, probably the same person who I see announced in the papers as Russian Consul-general, under the name of Gorkwitch, who accompanied the Russian admiral, Pontiatine, as naturalist, in his visit to Japan, when the treaty with that kingdom was entered into. M. Gaskevitch was wrecked in the Russian frigate Diana, in consequence of a terrible earthquake, and kindly treated by the Japanese, who subsisted the shipwrecked crew. Thereafter, in an attempt to rejoin the Admiral in a Russian ship, he was captured by the British, and his scientific career in these seas closed for the time by his having been sent home to be adjudged upon in this country; where he no sooner arrived than he was at once ordered to be set at liberty. He does not seem to have liked his captivity at all, and I am sure men of science in this country will like it as little. How any captain could have conceived it consistent with his duty to arrest a scientific man in his career of usefulness, under any circumstances, seems difficult to understand; but there are always two sides to a story—and *audi alteram partem* is a rule never deviated from without subsequent unpleasant reflections. If he has now returned to fill the important part of Consul-general, we may expect to reap a good harvest of Coleoptera through his friend Count Motschoulsky.

From the loss of his collections, sustained by M. Gaskevitch, the report and descriptions of the Japan and Amoor species are very meagre, chiefly Lepidoptera. Of the Coleoptera brought by him, M. Motschoulsky remarks that they offered little resemblance to our European species, and that there were only found three which appear identical with those of the west, *Anomala oblonga*, *Anobium paniceum*, and *Coccinella impustulata*. All the rest belonged to the type of China and the Philippine Isles.

If we have drawn little from the north of these seas of late years, the south has amply atoned for it; Mr Wallace having sent from Celebs, Aru, &c., a great variety of most beautiful and striking new species. These, for the most part, yet remain to be described; only a few of their favourite group, the Longicornes, having been described by Mr Pascoe in the *Transactions of the Entomological Society of London*; and by Mr Adam White, in the *Proceedings of the Zoological Society of London.* I hope we shall have many more of these species described by these gentlemen. The product of their pen is always clear and workmanlike. What they do they do well, and I wish we had more of it.

There has been an intermission to the rapidity with which the entomology of Australia and New Zealand was becoming known. Dr Baly's monograph of the genus *Australica* (the equivalent of our *Chrysomelidæ*) is, I think, the last connected description of species from that quarter. There is, however, a great mass of unappropriated material collected, and it is to be hoped that ere long some labourer may arise willing to cultivate the fields now lying vacant.

Passing on to South America, there is no work of any moment to record. Little groups of species, and some isolated descriptions, are all that I have to notice. One of the most interesting of these is the *Agrium fallaciosum* of Chev. (also described by M. Motschoulsky, under the name of *Pinochile cœnosa*). It is found at the Straits of Magellan, comes next to the *Manticora* of the Cape, and forms the transition between them and the *Omus* of the Rocky Mountains. It thus possesses a similar interest to the *Eucranid* of the deserts north of Patagonia, which represent the *Ateuchi* of Africa.

A curious genus, possessing the unique character of being viviparous, has been described by M. Schiodte as found in Brazil; and a good many of the new species brought to this country by M. Chabrillac have been described by Fairmaire and others in the *Transactions of the Entomological Society of France.* The contributions to the entomology of the Andes, in the neighbourhood of Quito, sent to this country by our correspondent, Professor Jameson, have been partially recorded by Dr Baly and myself, and a number of small species from Panama have been described by Count Motschoulsky.

Dr Leconte, a worthy son of a worthy sire, has taken entire possession of North America. He is the chief, indeed almost the only, entomological author now working in America; but he is a host in himself. He has reviewed most of the difficult families in the United States; his revisions, in fact, being singularly able monographs, evolving the most original views. He has thus gone over the *Cicindelidæ*, *Carabidæ*, including several of the *Amaræ*, the *Bembidia*, the *Hydrocantharidæ*, the *Palpicornes*, the *Buprestidæ*, the *Elateridæ*, the *Lamellicornes*, and the *Longicornes* of North America. He has, along with Dr Harris and Melsheimer, brought out the catalogue of species of Coleoptera in the United States, and has lent his hand in every quarter of the States to the advancement of zoological knowledge. I am here restricted to speaking of his doings in relation to the Coleoptera; but, were the time fitting, I might enlarge on his services in regard to almost every class of animals, from the Vertebrata downwards. In the Coleoptera, at an early period, we have from his pen descriptions of numerous species from California; we have descriptions of species from Texas; we have descriptions of species from Lake Superior—part of the report by Agassiz on that district; we have descriptions of species collected during the expedition sent to report upon the routes proposed for the railway across the Isthmus of Panama—all the product of his own collections, for his labours as a field entomologist have not been less than his talents and acumen as a closet naturalist. All this work has been done within the last few years, and it is still going on. Descriptions of new species from Kansas are in the press; and descriptions of new species from California will soon follow. We may hail Dr Leconte as one of the first living entomologists; and when we remember how scant a sympathy (comparatively only, of course, I mean) he has in his own country, the homage we pay him will only be the more hearty. Thanks to the Smithsonian Institution, we shall have most of Dr Leconte's works in our library.

A portion of North America, possessing special interest from its resemblance to a part of the opposite continent of Asia, I mean the Salt Lake region as compared with the Caspian district, has lately been somewhat opened. M. Lorquin, an able French entomologist, has made collections in that district, and they have reached Paris, and are in the hands of M. Boisduval, the lepidopterist. It is the Lepidoptera to which he chiefly restricts himself, and it is to them he specially refers in a notice of the collection given by him to the Entomological Society of France. He says, "Among these insects, many, although specifically new, have the aspect of those of the mountains of Europe, and especially of Siberia, several even are identical with some of our species."

Dr Asa Fitch, chiefly known as a zealous hemipterist has lately brought out, under the auspices of the State Government of Pennsylvania, a work on the noxious insects of that State; among which the habits of some Coleoptera are described. The care, accuracy, and perseverance shown in this work are very remarkable.

Such is a hasty, and, I am afraid, imperfect account of what has been doing in the science of Entomology (department Coleoptera) for the last three years. For its imperfections I now crave your apology; but, imperfect as it is, I think the impression which it must leave upon our minds is one of awe and amazement at the inexhaustible prolificness of Nature, and something also of admiration for the courage with which puny Man has set himself to the apparently interminable task of deciphering and recording such a towering pile of indigest materials.

3. *Report of the Committee on Marine Zoology.* By George Logan, Esq., Convener.

4. *A Specimen of the Canada Goose* (Anser Canadensis) *found in Duddingston Loch,* was exhibited by R. F. Logan, Esq.

Wednesday, 25th January 1860.—William Rhind, Esq., President, in the Chair.

The following Communications were read:—

1. *On the Reproduction of a* Medusa, *belonging to the genus* Lizzia. By Professor Edward Claparede, Geneva. Communicated by Dr T. Strethill Wright.

M. Claparède stated that he had captured, in September last, in Lamlash Bay, a number of floating eggs. On examining these eggs he found in each a true medusa, with four radiating gastrovascular canals, and eight tentacles—four short and four long, the long ones corresponding to the radiating canals. A careful search was rewarded by the discovery of the animal which had produced these eggs, a twelve tentacled medusa of the genus Lizzia; the bulbs, which in the embryo gave rise to the longer tentacles, being in the adult each furnished with two of these appendages. The peduncle was laden with eggs; of these eggs some exhibited a germinal vesicle and spot, others well-developed medusæ, but in none was the stage of segmentation of the yolk observed. The question was, whether the bodies in question were eggs or buds? It was true that no males of this form of Lizzia were found. But the males might be more rare than the females, or, as Dr Strethill Wright had observed in one case, might have a form different from that of the female. The structure of the bodies was that of true eggs. The canals of the medusæ which they contained had no communication with those of the parent, differing in this respect from the canals of the budding medusas of Sarsia. The buds of Sarsia, moreover, did not exhibit the germinal vesicle and spot. The author stated that the reproduction of medusæ, without the occurrence of a fixed hydroid stage, had been observed by Gegenbaur and Krohn, but in these cases the embroyos had to undergo important alterations in form before presenting the characters of the parent. M. Claparède considered it possible that reproduction in Lizzia might also take place with the intervention of the planuloid and hydroid stages.

2. *Note on an Instantaneous Method of finding Objects under High Microscopic Powers.* By T. Strethill Wright, M.D.

3. *Remarks on the* Musculus Kerato Cricoideus, *a Muscle of the Human Larynx.* By William Turner, M.B.

4. *Notice of the Capture of an enormous Cycloid Fish in the Bay of San Francisco.* By Andrew Murray, Esq.

This was a notice of an enormous fish taken at San Francisco. It was

360 pounds in weight, between seven and eight feet in length, and 5 feet 2 inches in girth round the body. It was supposed by its captors, who were probably New Yorkers, to be a giant specimen of the sea basse, or black basse, which is common on the east coast of America, especially about New York; but a scale of the fish, which had been sent home by Mr William Murray of San Francisco, showed that it was not a basse at all, nor any of the perch family. The scale was cycloid, not ctenoid, and the fish was more likely to have belonged to the sea-bream tribe of carps than to the sea basse. No fish of that magnitude belonging to these tribes seems hitherto to have been recorded.

5. *Dr John Alex. Smith exhibited a specimen of Bloch's Top-knot* (?), (Pleuronectes punctatus, Bloch), *taken near North Berwick; and of a Pipe-fish* (Syngnathus æquoreus, Yar.), *&c. &c.*

The specimen of this small flat fish of the genus Rhombus of Yarrell was sent to Edinburgh by Sir Hugh Dalrymple. It was taken in the beginning of August last, near North Berwick. There were two species of this genus described as closely resembling one another,—the *Rhombus hirtus*, or Muller's Top-knot (Yarrell); and the *R. punctatus*, or Bloch's Top-knot (Yarrell). But, unfortunately, the fins of this fish not being perfect made it difficult to decide to which of these so-called species it belonged. The upper surface, the left, and brown-coloured side of the fish was very rough, and the under surface was white, and also rough, though in a less degree, with the exception of the under side of the head, which was smooth. Muller's Top-knot was described as being *perfectly smooth* on the under surface, whereas Bloch's fish was rough; if this was a correct distinction, it would identify the specimen as being not Muller's but Bloch's Top-knot, which, as far as he was aware, had not before been observed in our neighbouring seas. Muller's fish was also rare; but specimens had been obtained at distant intervals in the Firth, especially towards its mouth. This fish measured about eight and a half inches long, by about five inches across, including the fins. Dr Smith also exhibited a specimen of a Pipe-fish, *Syngnathus* or *Nerophis æquoreus* (Kaup), measuring twenty inches in length. It was taken among the long weed on the coast of the Isle of May about three weeks ago. The dorsal fin, the only fin this species has, was about two and a half inches in length, and terminated nearly in the middle of the length of the fish, the vent being in a line with the beginning of the last fourth of the fin. It is of rare occurrence in our seas. Dr Parnell says, "this fish was first recorded as British by Sir R. Sibbald in 1685, who obtained a specimen in the Firth of Forth. No other instance of its occurrence in that locality has since been noticed. It is one of the rarest of our British fishes." He was indebted for the fish to Mr John Anderson of the Royal Emporium, George Street; and he had the pleasure of presenting it to the Museum of the University.

Dr Smith stated that Messrs John Dickson & Son, gunmakers, Prince's Street, had sent for exhibition two specimens of the stoat or ermine (*Mustela erminea*, Lin.). They were both nearly pure white, the point of the tail black; showing the severity of this winter. One was killed as early as the 27th of October, but the other not till the beginning of January. He also exhibited a male and female ruff (*Tringa pugnax*), shot in the neighbourhood of Carnwath in the beginning of September; and a young male shoveller (*Anas clypeata*), shot near Aberdour in the end of December. The keeper who killed it had never seen the bird before. Macgillivray, in his "British Birds," mentions that "in Scotland no authentic instance of its occurrence, at any season, has come to my knowledge." It has, however, been observed once or twice since his time. Dr Smith also exhibited two ducks, one killed in November last, on the coast of Mull, the other near Prestonpans, some weeks ago. Their unusual appearance had attracted attention, and gave rise to some correspondence in "The Field," one of the London sporting newspapers. The birds, Dr Smith said, were undoubtedly young females of the black scoter (*Oidemia nigra*, Flem.), the least common of our two scoters; and, in this immature plumage, rather a puzzling bird to a young naturalist. Their general plumage was of a dusky brown, the top of the head from base of bill and along back of neck brownish black; sides of head below eye, of throat, and neck, grayish white; the abdomen of a dull grayish brown, the brown feathers being edged with white; under tail coverts dark brown, no enlargement on bill, which, as well as the feet, was of a dusky brown colour. In one of the birds the bill was of a lighter colour at the nostrils. One of these birds was sent by Mr Sanderson, birdstuffer, and the other by Mr Small, birdstuffer, George Street. Through the kindness of Professor Allman, he was able to exhibit another immature specimen of this bird from the valuable collection in the Museum of the University. The occasional appearance of *Oidemia nigra* in this immature plumage has led, it is believed by naturalists, to the introduction, by mistake, of the *Oidemia leucocephala*, Steph., among the list of British birds. Jenyns says, there is no good authority for considering the *O. leucocephala* as British. It is a bird of eastern Europe.

February 1860.—T. STRETHILL WRIGHT, M.D., President, in the chair.

The following communications were read:—

1. *Notice of various Osteological Remains found in a Pict's House in the Island of Harris.* By JAMES M'BAIN, M.D., R.N., (The specimens were exhibited.)

Dr M'Bain said, that the fragments of bone which he exhibited were brought from the Island of Harris, by Captain Thomas, of Her Majesty's surveying vessel Woodlark. They were found during last summer in one of those interesting buildings, commonly called "Picts' Houses," which was opened at a place named Nisibost, in the Island of Harris, for the purpose of extending former observations made by Captain Thomas upon these ancient structures. The fragments of bone had been put into the hands of Dr M'Bain, in order to determine to what species of animals they belonged. And as there is historical evidence that the antiquity of "Picts' Houses" extends at least beyond a thousand years, he thought the remains of animals preserved in these buildings worthy of being considered zoologically in reference to the extinction or extirpation of species. An anatomical description of the bone fragments, fifteen in number, was given. They belonged to the following species of animals:—The dog, the common seal; the red deer (part of the antlers of which had been cut and fashioned by a sharp instrument); the "Bos longifrons," characterised by the form of the cancellous horn-core, which formed one of the specimens; the sheep, of small size; and the right middle metacarpal or cannon bone of a small horse, rather larger than that in the skeleton of a Shetland pony in the Barclaian Museum of the Royal College of Surgeons of Edinburgh.

2. *On the Structure of Pearl.* By ALEX. BRYSON, Esq. (Numerous illustrative specimens were exhibited.)

The author commenced by stating that the first mention of pearls being used as ornaments by mankind was found in the ancient writings of the Chinese. So early as twenty-two and a half centuries before the Christian era pearls are enumerated as tribute or tax. In the Rh—ya, a dictionary compiled one thousand years before Christ, pearls are mentioned among the most precious products of the empire. Grill, a Swede, long resident in China, was the first who published an account of the Chinese method of forming artificial pearls. This interesting paper is published in the Transactions of the Royal Swedish Academy for 1772. He says—When the shells (the *Unio plicatus*) rise to the surface of the water to sun themselves, they open their valves. The Chinese, watching their opportunity, insert between the mantle and the shell a string of coarse, ill-coloured pearls, placed at intervals on a cord or wire. When these are inserted, the shells sink to the bottom of the pond, where they are allowed to remain for one year, when they are fished up and opened; the coarse rough pearls are now found coated with a fine covering of nacre. In the joss shells are placed clay images of Buddha, which, when sufficiently covered with nacre, are skilfully sawn out by the Chinese, and worn and worshipped by them as the emblem of the creative power. Linneus, probably unaware of what had been done in China so many hundred years before our era, endeavoured to produce artificial pearls by piercing the nacreous shells from without, and inserting foreign bodies; but his success was not so great as his patron, King Frederick Adolphus, had anticipated. So sanguine was his Swedish Majesty that that discovery would enrich his country and decorate his court, that he conferred a pension and a patent of nobility on the great naturalist. Had this honour been conferred on Linneus for his "Systema Naturæ," the monarch would have been more honoured, and the conferred title of Von Linné perhaps respected by posterity. Unfortunately for the monarch, his empty title is forgotten, and Linneus, not Von Linné, remembered with veneration by all true lovers of nature. Mr Bryson remarked that, though the French are now by far the most successful producers of artificial pearls, he had failed to obtain the slightest hint of the method employed, no paper having appeared, as far as he was aware, on the subject. The only notice of the formation of the *coques de perles* of the French which he had obtained was by Von Siebald, who has given, in his "Zeitschrift für Wissenschaftliche Zoologie," a description of the process. It differs very little from that followed by the Chinese. A piece of nacre is sawn from a shell of the required form, and placed between the mantle and the shell of a nacre-producing mollusc; when sufficiently coated, it is filled with mastic, and a small plate of mother-of-pearl placed at the back. In regard to British pearls, the author stated that the first notice of the gem was by Tacitus in his "Life of Agricola;" and that the pearls were the product of the fresh-water mussel of our rivers (*Unio margaritafera*), was evident from the description, that they were "not very orient, but pale and wan." To the theory advanced by Arnoldi in 1696, anew by Sir Everard Home in 1818, and also by Kellart in 1858, that pearls, or rather their nuclei, were due to the sterile ova of the molluscs which produced them, the author gave his decided opposition, as, from all the facts which he had observed, pearls were entirely due to a secretion from the mantle of the animal. To illustrate the structure of pearls, Mr Bryson exhibited a large series of sections which he had prepared, and by which he showed that by the microscope he could at once determine what shell had produced them. He also explained the rationale of the irridescence of mother-of-pearl,—a discovery due to Sir David Brewster, who proved that it was due to the diffraction of the rays of light, caused by the outcropping edges of the laminæ, and in some cases to the minute plication of a single lamina. This phenomenon was also shown by Barton's patent buttons, where the irridescence was produced by thousands of minute lines, so near each other as to require a high magnifying power to resolve them. By taking an impression with black wax under considerable pressure, the author succeeded in obtaining the same irridescence as exhibited by the button itself. This experiment Sir David Brewster had tried with success in 1815, by taking an impression in wax from a mother-of-pearl button, and by which he demonstrated the cause of the phenomenon. The commercial value of pearls, the author stated, was still as high as in the days of Cleopatra. A good Scotch pearl, with fine lustre, of the size of a pea, fetches from L.3 to L.4. The famous wager between Antony and Cleopatra gives us an insight into the value of pearls. The two pearls which that luxurious Queen resolved to disolve in vinegar, and serve up at the costly banquet, were valued at ten million of sesterces, about L.76,000 sterling. The pearl in the possession of Mr Hope, M.P., the largest of modern times, is not worth a fourth of that sum. The weight of this pearl is 3 oz.; it is 4½ inches in circumference, and 2 inches in length. Notwithstanding the great value of the pearls, the shells of the animals yield now a far more profitable return than the jewels. In 1856, the total value of the pearls imported into this country was L.56,162, whereas the imports of 2102 tons of mother-of-pearl shells were valued at L.76,544. Mr Bryson suggested that trials should be made to produce artificial pearls from the Iridina, a nacreous shell, having a much higher lustre than any hitherto found. It inhabits the Nile and Senegal rivers.

3. *Notes of the Chough or Red-Legged Crow* (Fregilus graculus); *on the Migration of the Swift* (Cypselus apus, Flem.); *and on the Effects of the severe Gale on the 9th September last.* By the Rev. Thomas B. Bell, Leswalt, Wigtonshire. Communicated in a letter to Dr J. A. Smith.

Mr Bell, in his communication, says,—"This bird is common all along our rocky shores, building on cliffs and in caves along with his mischievous companion the jackdaw, and sometimes in the same cave with the rock pigeon. He annoys the farmers by digging up the sprouting wheat, and tearing up the roofs of their stacks. He is not by half so wary as either the rook or jackdaw, and, consequently, falls a frequent victim to the herd-boy's gun. He is a pretty bird, very easily domesticated, but void of genius. He does not care to congregate, is not clamorous, and never goes far inland—perhaps not above a mile; but he shifts his roosting quarters frequently from one cave or rock to another, probably just because the wind shifts.—On the 19th of July last, I observed a migration of several hundreds of the common swift. I knew that these birds, breeding only once in the season, were the first of the swallow tribe to leave our shores, but I had no idea that they left us so early. These are the first I have seen here during a residence of nearly nineteen years. Perhaps I may mention the particulars of this flitting in as few words as possible. 1st, The weather was very warm and still, with a few fleecy clouds overhead. The hour was between five and six in the afternoon. 2d, The direction from which they came was N.W., as I thought—probably from Ayrshire. They passed over me as I stood on the shore, at a spot about five miles north of Portpatrick, and, holding on their way, I judged that they would reach the Irish coast somewhere about Portaferry. 3d, Their flight was direct, and steady, and quiet—no wheeling nor screaming, such as they practise when feeding or sporting round some old grey tower. They seemed to have important business on hand, and went about it in a businesslike way. The level of their course was not high. They swept over the cliffs, which are not over 150 feet, and seemed to retain the same level, as far as I could see them crossing the Channel. I should guess the height at about 250 feet, or even less." Mr Bell then describes their order of flight, and says,—He estimated their number at nearly

1000.—"On Friday, the 9th September, we had," he continues, "our first equinoctial gale. It lasted for about a week. On the 10th I picked up (at the place where I had seen the swifts in July) a stormy petrel, the *Procellaria pelagica* of the Atlantic. These birds were frequently seen off our shores in former times; but now they seem to keep outside of the Mull of Cantyre. Whether an increase of steam navigation has driven them from the Firth of Clyde to the open ocean, I cannot say. The same gale cast ashore a vast number of medusæ; each tide left a belt of them, ten or twelve feet wide, all along the beach. The most remarkable result of the gale, however, was the destruction of many thousands of the short-winged sea fowls. From Corsewall Point to the Mull of Galloway, all round the shores of the Bay of Luce and of Loch Ryan, razor-bills and guillemots were lying in heaps. I did not observe any other species. Want of food for several days, combined with the exhaustion induced by struggling so long against a head sea, seems sufficiently to account for the prodigious mortality among them. This is confirmed by the fact that no gulls nor gannets perished. The former easily obtained shelter and food on shore, while the latter, with their tremendous power of flight, escaped to sheltered bays and coasts."

4. (1.) *Notice of a New Leaf Insect.* By ANDREW MURRAY, Esq.

Mr MURRAY exhibited a beautiful photograph of the underside of a butterfly, in every respect exactly like a dead leaf. He had received it from Dr William Traill, H.E.I.C., presently stationed at Russelcondah in the Madras Presidency. Dr Traill, in transmitting the photograph, writes:—"I wished to have sent you a curious insect, brought to me as a leaf insect. In Singapore and the Straits, where a variety of these singular forms are found, they are all allied to the *Orthoptera*, or the genera *Mantis*, *Empusa*, *Phasma*, &c. I am a good deal accustomed to their various forms, but on this occasion I was completely taken in, and until the animal moved, I thought it a dead leaf. To my surprise, I found it to be a butterfly! When at rest, its two anterior wings (which are slightly falcate at the tip) were pushed forward in front of its head, so that a central line on them exactly met a similar central line on the posterior wings, so as to simulate the mid rib of a leaf. The four wings so disposed presented the most exquisite resemblance to an autumnal leaf; and even the veining is represented with wonderful fidelity, especially if the animal is held two or three feet from the eye of the spectator. A remorseless rat one night carried off the insect, along with the pin on which it was impaled; but I had a few days before got a photograph of it made, which I now send you. It is, however, very far from giving a just idea of the original. The upper side of the wings were most brilliantly coloured, but I do not remember exactly what colours." Of course, these brilliant colours will only be seen when the insect is in motion; when at rest, and more exposed to danger, the folding back of the wings conceals them, and shows only this extraordinary resemblance to a leaf. The resemblance is every whit as great as that exhibited by the leaf insect proper (*Phyllium*), only being that of a dead leaf instead of a green one. The insect appears to be undescribed, and, from its powers of concealment, is no doubt rarely captured. Most butterflies have lines on the anterior and posterior wings, often both above and below, which become continuous when placed in juxtaposition; and there are several exotic species which have a line similar to the mid-rib of a leaf figured upon the under side of the wings; but none hitherto described at all approach the present in its close resemblance to a leaf, both in shape, veining, and shading. It is impossible, from merely a photograph of its underside, to determine its genus; but from its falcate anterior and single-tailed posterior wings, it probably belongs to the same group of the *Nymphalidæ* as *Amathusia* and *Zeuxidia.*

(2.) *Description of New Sertularidæ, from the coast of California.* By ANDREW MURRAY, Esq.

This paper consisted of a scientific description of certain new species of *Sertularidæ* from the Bay of San Francisco. The chief point of interest in it was the close resemblance which they bore to the species of *Sertularidæ* found on the coasts of Britain.

(3.) *Notice of a New Species of Chameleon.* By Mr MURRAY.

This is a curiously-formed species of chameleon, brought from the interior of the Old Calabar district of West Africa, by one of the natives, to the Rev. Mr Baillie, by whom it was presented to Mr Murray. It is characterised by three salient horny processes on the head. Many lizards have singular spiny projections on all parts of the body.; but this very well marked species has not been hitherto recorded. In allusion to the prongs on the head, Mr Murray named it *Chameleon tricornis.*

5. Dr SMITH exhibited a Ballan Wrasse (*Labrus bergylta*), caught in October last in Loch Fyne. It was sent by Captain J. H. P. Orde, of Kilmory, Lochgilphead, Argyllshire.

6. Dr SMITH also exhibited a specimen of a female Gadwall duck, *Querquedula strepera,* one of our very rare winter visitors. It was shot near Cromarty in the end of January, and was kindly sent to Dr Smith, by Mr Muirhead, Queen Street. He referred to the great abundance of the Brambling, *Fringilla montifringilla,* and of the Siskin, *F. spinus,* during the severe weather of this winter: and also to the multitudes of wood pigeons, *Columba palumbus,* which, from stress of weather and starvation, had been driven from the more open and wooded districts to the neighbourhood of our gardens and towns, and had eaten up, with the exception of the leek, all kinds of garden produce.

March 1860.—ALEXANDER BRYSON, Esq., President, in the chair.

The following communications were read:—

1. *Observations on the Microscopic Structure of the Human Pancreas.* By WILLIAM TURNER, M.B., Demonstrator of Anatomy in the University.

2. *Notice of Reptilian Fossils, Morayshire.* By WILLIAM RHIND, Esq.

The specimens which Mr Rhind exhibited of reptilian remains from the sandstone of Moray, were contributed by Patrick Duff, Esq. It is now about a quarter of a century since the late lamented President of this Society, Professor Fleming, first detected an organism in the Old Red Sandstone of Scotland. It was but a minute fragment of a fish scale, yet it had the effect of awakening an interest for, and stimulating a research into, those beds of sandstone which skirt almost the whole of the Scottish shores to the north of the Firth of Forth, and which hitherto had been looked upon as destitute of organic remains. A few years after this discovery, Mr Duff began his researches in Morayshire, with an enthusiasm and perseverance which have seldom been equalled. He soon found that the sandstones of Moray teemed with organic remains. From that period he has formed the nucleus round which the researches of other scientific men and the casual discoveries of the workmen in the various quarries have centred, so that a most varied and interesting assortment of specimens have been accumulated. As long as the organisms brought to light partook of the character, or were supposed to do so, of fishes, no doubts remained that the sandstones of Moray, under the several modifications of colour and position, belonged to the Devonian era; but subsequently, when organisms of a higher order made their appearance, assuming the distinct forms of reptiles of various families and sizes, a doubt began to arise whether the fish-bearing and the reptile-producing strata belonged to the same series. This *questio vexata* still prevails; and Mr Rhind, assuming that the decision is still left open, proceeded to exhibit by a section the relative positions of the sandstone beds, in so far as these are open to inspection. The general conformity of the lowest red, the greyish, and the yellow sandstones,—the parallelism of the dip of these three beds of strata, and the superposition of a band of limestone or *cornstone* commencing south of Elgin, and seen with more or less interruption at Linksfield, Spynie, and Stotfield, capping and inclosing the whole series, were pointed out; while the absence of scales of the distinctive fishes of the Devonian era in the particular localities where the reptilian remains have been discovered was also mentioned,— an absence which probably may arise from as yet defective search, considering that the accidental disinterment of the reptiles has occurred within the short space of a few years, and that another few years may either add the discovery of fish scales, or, if not found, afford a somewhat negative proof of the non-identity of the yellow sandstones with the Devonian. It was also suggested that the appearance of the vast masses of compact sandstone forming the hills which traverse the lower region of Moray from west to east, indicated a process of accumulation by drifting, rather than that slow and regular deposition which is indicated in the lower red sandstone beds, where conglomerate, shale, and fine-grained sandstones are alternately superimposed,—a condition always favourable to the existence of organic remains either of plants or animals; while the drifting process, which carried the land reptiles into the sea, was unfavourable to the preservation of fishes. Lastly, allusion was made to the recent discoveries of vegetable remains of Lepidodendrons, Lycopodiums, and others analogous to those of the coal strata in the true Devonian sandstones of Canada, by Mr Dawson, and of similar fossil plants found in the Caithness slates, by Mr Salter,—all indicative of dry land, and the progress of organic life during the period of the Devonian Sea.

3. *Contributions to the Natural History of Old Calabar.* By ANDREW MURRAY, Esq.

4. *On the Chalk Flints of the Island of Stroma, and Vicinity of John o'Groat's, in the County of Caithness.* By CHARLES W. PEACH, Esq., Wick.

I had to go to the Island of Stroma, and when walking across the north end of it, I was somewhat surprised to find chalk flints in some abundance on the surface. This part of the island is stripped of its turfy covering, consequently favourable for observation; and wherever I went, even on the ploughed land, and where not too much covered with vegetation, I met with them. The flints vary in size from that of a boy's marble to eight or ten inches square, and are generally of a light colour. Some contain sponges; in most of them spiculæ may be seen in the thin splinters which I chipped off, and in one piece those hollow and radiated forms with the small ball-like masses peculiar to the genus *Geodia*; all beautiful objects for the microscope. A large one of several pounds weight contained pieces of shells, a fragment of an *Echinus*, and spiculæ of sponge. They are slightly water-worn; many covered with lichens. With them are blocks of granite and gneiss, some of large size, with smaller pieces of hornblende, reddish conglomerate, and quartz.

(2.) *Note of the* Onuphis tubicola, *found near Wick.* By C. W. PEACH, Esq.

Wednesday, 25th April 1860.—WILLIAM RHIND, Esq., President, in the Chair.

The following Communications were read:—

1. *On some Obscure Markings upon an Old Red Sandstone Slab at Mill of Ash, near Dunblane.* By the Rev. ROBERT HUNTER, late of Nagpore.

2. *Notices of Various Ornithic Fragments of Fossil Bones from New Zealand.* By JAMES M'BAIN, M.D., R.N.

The bones were found in a limestone cave in the northern island of New Zealand, and sent home by Dr A. S. Thomson of the 58th regiment. The skull was unknown to Dr Thomson, and differed from all the moa's skulls that he had seen; at the same time, he thinks it belongs to the Genus *Dinornis.* The large bones were nine in number, consisting of the greater portion of a cranium, one cervical, one dorsal, and nine anchylosed sacral vertebræ, a part of a rib, an ungual, and a corresponding penultimate phalanx, a large elongated bone, which he assumed to be a scapula, and an interesting little oval bone, doubtless a tracheal ring. The bones were of a yellowish cream colour, light and spongy from the loss of animal matter, but all in good state of preservation. There were none of the mandibular or facial bones attached to the cranium, a part of the presphenoid, and the whole of the postorbital process of the frontal bone were broken off, and there was likewise a slight exfoliation at the supraoccipital and paroccipital ridges, exposing the remarkably cancellous structure of the cranium. In the description of the skull from Rotomarrama, Dr M'Bain adopted the comparisons made by Professor Owen on the skull of *Palapteryx geranoides.* There is a mutilated cranium of a Palapteryx, which was sent to Professor Owen by the Rev. W. Cotton, from the north island of New Zealand, described and figured in the third volume of the Transactions of the Zoological Society, p. 360 (plate 55, figs. 4 and 5), without any specific name assigned to it, which is closely allied to, if it be not identical with, the Rotomarrama cranium. It is said to be "equal in size to that of *Dinornis casuarinus;* and from the presence of the left postorbital process, to furnish another mark of difference from the cranial structure of *Dinornis* proper, namely, the non-union of the postfrontal with the mastoid."

3. *Notice of the* Angwántibo *of Old Calabar; an animal belonging apparently to the Genus* Perodicticus *of Bennet.* By JOHN ALEXANDER SMITH, M.D.

The animal was received some time ago from the Rev. Alexander Robb, one of the United Presbyterian missionaries at Old Calabar. Dr Smith read the following extract from a letter, dated Old Calabar, 1st December 1859:—"I was at Creek Town yesterday, and received from 'King Eyo Honesty' a small bush animal. It seems to be a *lori,* or *Stenops tardigradus.* The Calabar people call it *Angwántibo.* It lives in trees; but, being nocturnal, the people know exceedingly little about it. The Rev. H. M. Waddel says that the "*Angwántibo* is an animal of the sloth kind, that it lives in trees, hangs on the branches, and eats fruit. It is rather larger, when full grown, than a large cat, with a longish snout, short ears, each foot three long crooked toes, and claws with a thumb similarly shaped, and no tail. It is dun-coloured, and cannot walk on the ground, but when set down, crawls a little, falls over, and rolls itself up in a ball. It is inoffensive." The animal belongs to the Family of the *Lemurs*, and to the Genus *Stenops* (Illig.), or rather that division of it called by Mr E. T. Bennet, the genus *Perodicticus*. It seems to differ, however, from the only species described, the *P. Geoffroyi* (Bennet), or *Stenops potto* the *Aposo* of Guinea; the relative proportions of its different parts not agreeing with this individual. So that, while in want of information, and being unable to procure various works for examination, Dr Smith was inclined to consider it a new species, and names it provisionally the *P. Calabarensis*. Its dentition is, incisors 2 2, 2 2: canines, 1-1, 1-1; molars, 6 6, 6 6 = 36. The animal is a male, it measures 11¼ inches long from snout to point of tail, which is only a quarter of an inch long, and is hid in the long woolly-like uniform greyish brown, or dun-coloured hair of the body; the limbs are nearly equal in length. The thumb of the forehand has at its inner base a large rounded tubercle, and opposed to it is the undeveloped index finger, which projects only about one-eighth of an inch, has no nail, and is supported apparently by a metacarpal bone only; the other three fingers and thumb have flat, rounded nails, as well as those of the hinder hand or foot, except the index finger of the latter, which terminates in a projecting claw. In each foot the thumb, with the tubercle at its base, is opposed to the rest of the fingers, forming in this way a powerful instrument of prehension. The tongue has the curious bird-like tongue below it, projecting forwards from the frenum, and terminating in nine pointed filaments.

Wednesday, *9th May*.—T. STRETHILL WRIGHT, M.D., President, in the Chair.

The following communications were read:—

1. *On the Silicification of Organic Bodies, and on Beekites.* By ALEX. BRYSON, Esq. (Specimens were exhibited.)

2. *Observations on British Zoophytes.* By T. STRETHILL WRIGHT, M.D.

The author stated that, in the summer of 1858, he took, by dipping, a great number of medusæ of the genus Thaumantias, off Granton Pier. To the peduncle of one of these was attached a small actinia, about half an inch in length, and one-eighth of an inch in diameter. From its general appearance, he considered it to be a young specimen of *Actinia troglodytes*, which had been seized by the medusa, dragged from its native mud, and brought captive to the surface of the water; but it was unfortunately lost before he could examine it carefully. In June, his friend, Mr Fulton of Granton Pier, brought him a number of Thaumantias, to one of which another actinia, of the same species as the one he had before observed, had attached itself by swallowing the peduncle of the medusa. The body of this actinia was of a transparent, yellowish-white colour, and marked by twelve paler lines, indicating the situation of the longitudinal septa within. The oral disc was oval, and formed by the basis of the tentacles and the mouth. The tentacles were twelve in number, of a rich umber brown colour. About one-half of each from the base was marked with five opaque pale-yellow lozenges, and from thence to the top by four bands of the same pale-yellow colour. The brown matter consisted of amorphous, pigment granules, the yellow matter of highly refractive and exceedingly minute molecules, apparently calcareous. Each tentacle was curved backwards, and resembled the abdomen of a wasp. The pigment was forced through the top of the tentacle by pressure, indicating an opening at that part. The mouth, instead of being linear, as in the actinias, tended to assume a quadrangular, or crucial form, though the constantly varying shape of the disc rendered a description of it difficult. The stomach was very peculiar, and differed from that of the actinias. It was a flat and obscurely quadrangular sac. Its angles he should describe as superior, lateral, and posterior. The superior angle was connected to the parietes of the body by four septa, the lateral angles each by one septum, and the posterior angle by two septa. These septa were continued downwards, as in the actinias, to the lower extremity of the body, and had their free edges bordered by a convoluted ciliated band, furnished with enide, or thread-cells. The stomach and parietes were further connected by four intersepta, as he should call them —one between each of the lateral and anterior angles of the stomach, and one between each of the lateral and posterior angles; but these intersepta bore no convoluted bands. The septa probably bore ovaries, or spermaries, the intersepta not, in which case the reproductive system of the animal now described agreed in simplicity with that of the polyp of the Alcyonidæ, which had only eight septa, each bearing ciliated bands. The upper part of each of the septa and intersepta was perforated by an oval opening, so as to give an uninterrupted passage beneath the tentacles to the circulation of the fluids of the body. By tracing this passage in the Lucernarias, he had come to the conclusion that it was the homologue of the circular c~~or~~al of the gymnopthalmatous medusa. \\ an The attachments of the stomach thus resembled those of the same organ in the other Helianthoid and Alcyonian polyps, but in shape it widely differs from these. In Act~~u~~ia and Alcyonia the stomach was a \\ in flattened sac, open, and evenly truncated at its lower extremity. In the animal now described the lower border of the stomach curved gently downwards from the superior to the lateral angles, and from the lateral to the inferior angle it bent deeply and abruptly downwards, while the last-named angle itself was produced outwards and downwards, so as to form a beaked process, as shown in the figure before the Society. The threadcells of the tentacles are simple and unbarbed; those of the septal bands furnished with a zig-zag thread, as in figure. When the animal was separated from the peduncle of the medusa, and placed in a dish of sea-water, it slowly moved from place to place by the aid of the tenacious palpocils which studded the tentacles and upper part of the body, and alternately emptied itself like a balloon, and emptied itself by a vermicular contraction of the parietes, which commenced beneath the tentacles, and passed backwards. When dilated, it was seen that the animal was destitute of a sucking disc, and that the posterior part of the body terminated in a funnel-shaped depression, opening into the cavity of the body, and permitting ingress of water therein. During contraction, this funnel was everted, and became a cone, through the apex of which the fluid was again ejected. This animal resembled the *Actinia crysanthellum* of Peach, now classed by Gosse under the genus Peachia, but it differed from it in the markings of the tentacles, and in the absence of the tubercles, placed between the exterior of the bases of the tentacles. In the present species these tubercles are replaced by a faint tinge of yellowish pigment. A further examination of the stomach of the known species of Peachia must determine whether the present animal belongs to that genus. In the meantime, the author proposes to call it *Peachia Fultoni.*

3. *On New Fossil Forms from the Old Red Sandstone of Forfarshire.* By David Page, Esq. (Specimens of the fossil fish were exhibited.)

Mr Page next drew attention to some new fossil forms from the Old Red Sandstone of Forfarshire. These fossils occur in a bed of highly fissile shale, lying in the course of the Powburn, near the church of Farnell, and belong to the grey tilestones, or lowermost series of the system. They consist chiefly of fishes and crustacea—the former embracing three or four species of *Diplacanthus*, two of *Acanthodes*, *Cheiracanthus*, and several forms yet undescribed; the latter being *Pterygotus*, *Eurypterus*, and *Kampecaris*, with detached plates, and *Parka decipiens*.

4. *Notice of Snakes and Lizards received from the U. P. Missionaries at Old Calabar, Africa.* By George Logan, Esq., W.S.

5. *On the Nidus and Young of* Pontobdella muricata, *and other Annelides.* By Chas. William Peach, Esq. Wick. (With illustrative sketches.)

Botanical Society of Edinburgh.

Thursday, 9th February 1860.—Professor Balfour, V.P., in the Chair.

The following communications were read:—

1. *Biographical notice of the late Dr Gilbert M'Nab of Jamaica.* By Professor Balfour.

It is our melancholy duty this evening to record the death of Dr Gilbert M'Nab, one of the Fellows of this Society. The sad event took place at St Ann's in the island of Jamaica on the 21st January last. Dr M'Nab was a son of the late and brother of the present superintendent of our Botanic Garden. He was born in the parish of St Cuthbert's, Edinburgh, on the 26th November 1815. After prosecuting his elementary studies, he became a student in the University of Edinburgh, and devoted his attention to medicine. He graduated here in 1836 along with 122 other medical students, and he wrote a thesis "on the Botany of the Coast of Forfarshire." After his graduation he became an assistant in Dr Christison's laboratory. Here he acquitted himself to the entire satisfaction of the Professor; and he became a favourite with all his companions. He was fond of botany, and he prosecuted the science with great zeal and success. He was one of the 21 (of whom 12 remain) who met on the 8th February 1836 to institute the Botanical Society; and on the 27th March of that year he was enrolled as one of the original members. He rendered important services to the Society; and I find that on 13th April 1837 the thanks of the Society were given to him for valuable services and unwearied exertions in conducting the distribution of the specimens of 1836–37. Dr M'Nab made many excursions in Scotland, and communicated the results of his observations as well as specimens to the Society. His botanical tour in Forfarshire was given in his thesis. He visited also the county of Galloway and the Orkney and Shetland Islands. Among some of the rare plants collected by him and presented to the Society, I may mention the following:—*Arenaria norvegica*, from Serpentine Hill, Unst, Shetland; *Cerastium latifolium*, var., from Shetland; *Ajuga pyramidalis*, Orkney; *Allium oleraceum*, near Montrose; *Asplenium germanicum*, Dunkeld; *Calamagrostis Epigejos*, Braemar; *Hieracium umbellatum*, Clova; *Rhinanthus major*, Sands of Barry.

An opportunity for engaging in practice having opened at St Ann's, Jamaica, he left Edinburgh on 9th January 1838. Before his departure the members of the Society testified their regard for him by inviting him to a supper in Barry's Hotel, on 27th December 1837—Professor Graham occupying the chair, and Professor Christison acting as croupier. Every one felt regret at the loss of his valuable assistance in conducting the affairs of the Society.

After being at St Ann's for some time he was called to Kingston to act as assistant to Dr M'Fadyen, an eminent medical practitioner as well as a good botanist. Dr M'Nab aided Dr M'Fadyen both in his practice and in drawing up his Flora of Jamaica, of which unfortunately only one volume has appeared. Amidst the arduous duties of practice in the warm climate of Jamaica, Dr M'Nab did not neglect the pursuit of botany, and he contributed largely to the Society's Herbarium. The plants which he transmitted are incorporated with the University collection, and many of them have been transmitted to Dr Grisebach, who is now engaged in a work on the West Indian flora. Dr M'Nab also contributed largely to the museum at the Botanic Garden. Some of the specimens he has sent are very valuable and instructive. The difficulty of transmission prevented him from forwarding many large specimens of palm stems, &c., which he had secured for the museum with much trouble and expense.

In January last, he was attacked with inflammation of the kidney, accompanied with a cervical abscess, which appears to have burst internally, and caused sudden suffocation. His remains were interred in the church of Ochro Rois, St Ann's.

A writer in a local paper, in recording his death, says:—"There are few men in Jamaica whose demise will cause more sincere or more general regret than that of Dr M'Nab, who united to eminent skill as a surgeon and general practitioner one of the kindest and most amiable of dispositions. He practised for many years in Kingston—first, in partnership with the late Dr M'Fadyen, whose botanical tastes found a large sympathy in the cultivated acquaintance with that charming science which Dr M'Nab had acquired from his earliest associations—and subsequently on his own account, in the course of which he made numerous friends, all of whom will deplore his early death."

Dr M'Nab communicated a paper on the *Nelumbium luteum*, and he was the first to introduce the *Victoria regia* into Jamaica.

2. *On the Phytotype or Archetype of the Flowering Division of the Vegetable Kingdom.* By J. Birkbeck Nivens, M.D. (Lond.), Fel. Bot. Soc. Ed. Communicated by Dyce Duckworth, Esq.

The author commenced by describing the theory of Morphology as announced by Goethe, and mentioning the principal facts upon which it is based. He spoke of the advance in botanical knowledge which had resulted from the discovery of the law of Morphology, and the obligation under which botanists lay to the illustrious poet. He then alluded to the objections naturally felt to the theory in the form in which Goethe propounds it—viz., "that every part of a plant was a modification of a leaf." He mentioned plants which have no leaves, but consist entirely of stem or flower, and pointed out the comparative non-importance of the leaves, whose functions were frequently performed by the stem, and on the other hand the essential importance of the reproductive organs, the stamens and ovaries, so that it appeared unphilosophical to consider an essential organ as a mere modification of a non-essential one. A leaf also, in ordinary language, was well understood to mean an organ more or less green, and possessing other characters popularly well known; and it seemed erroneous to talk of a part like a stamen or an ovary, both equally distinct and well defined with a leaf, as a mere modification of such an organ. He therefore proposed to see if there were not some simpler and more elementary type upon which the various parts of a plant were constructed; and he conceived that this would probably be found in an internal structure, rather than in a mere external form, such as that of a leaf. Such a type the microscope would probably have revealed to Goethe had it been in common use in his day, and such a type he now proposed to bring before the Society.

The most elementary form of the type was likely to be met with in a simple rather than in a complex organ; and in searching for the simplest part in a perfect flowering plant, the so-called *abortive* stamens of the Erodium, the Stork's bill, were selected as being the most likely, from their extreme simplicity, for they consist of nothing but a delicate tapering filament, which is so thin as to be rendered transparent by the slightest pressure between two pieces of glass. When examined under the microscope the entire structure was found to consist of a single central spiral vessel, surrounded by long tapering cells. Here, then, was an entire organ of a perfect flowering plant, which consisted of nothing but a *spiral vessel surrounded by cells;* a structure which was probably, therefore, the simplest form of the type upon which flowering plants are constructed. On examining more complex organs the same typical form was met with under various modifications which were easily traced. Thus the stem of the Dodder, which is in fact the entire plant for the greater part of the year, was found to consist merely of two or three spiral vessels running parellel with each other, and surrounded by two rows of rectangular instead of tapering cells. The corolla of the lily of the valley presented six separate spiral vessels running parallel with each other, but connected by delicate cells, which formed the expanded portion of the corolla. As this flower belongs to the endogens, a ternary arrangement might naturally be looked for; and the typical form of "a spiral surrounded by cells" was found to be repeated in it twice three times. In like manner the single style of the hyacinth (also an endogen) was found under the microscope to consist entirely of three separate spirals, running parallel with each other, but connected by surrounding delicate cells. In the tube formed by the united filaments of the nine stamens in the Leguminosæ, there were nine separate spirals, each terminating in an anther, but connected by still more delicate cells where they form the tubular sheath of the ovary. A young leaf of the Callitriche, which was also rendered transparent by pressure, differed from all the foregoing in the circumstance that the spiral vessels were ramified, and looped like the veins of an exogenous leaf generally, and that the cells were filled with green pigment; but in every other respect the same typical form was present—viz., a spiral vessel surrounded by cells.

The next subject for investigation was to ascertain at what period of the life of the plant this structure makes its appearance; for the ovules are at first merely cellular bodies, and the pollen grains contain no spiral vessels. A large number of embryos were examined, including the pea, the mustard, the melon, castor oil, the almond, wheat, nux vomica, and others, but in *no single instance could any trace of a spiral vessel be discovered before germination commenced*, although they became apparent in every case after the embryos had begun to sprout. The order of their appearance in the plumule and radicle was not perfectly uniform, but very nearly so. In almost every instance the spiral vessels were first observed in the radicle, and only at a later date in the plumule, and the exceptions were so rare as to leave no doubt that the law of development is that the spirals are first formed in the radicle, and afterwards in the plumule. The spiral vessels were always traced with ease into each of the rootlets into which the radicle divided in forming a root; but they continued unbranched and separate throughout their course. In the plumule, on the contrary, there was at first a single undivided spiral vessel running up the centre of the cotyledonary leaf; and as this increased in development the spiral became multiplied, though still unramifying; and at last it ramified after the manner of the veins in an ordinary exogenous leaf. In the plumule of endogens the spiral became multiplied as growth advanced, but the vessels continued parallel with each other and unramifying.

As the seedling progressed still further toward maturity, the cells surrounding the vessels underwent various modifications requisite for the purpose they were eventually to serve. Thus they became filled with green pigment, and lost their transparency in the strong deep green permanent leaves; in the stem of herbaceous plants they were elongated and tapering in some cases, and in others elongated and rectangular; in the delicate petals and still more slender stamens they were of corresponding delicacy, and were either colourless or filled with the colouring pigment of the organ; whilst even in the earliest stage of the dense, hard, woody embryo of the acorn, the cells were so thick, strong, and opaque, as to render division by a sharp instrument necessary before they became even semi-transparent; and even then they obscured the spiral vessels so much as to make it impossible to trace them without interruption. The conclusion, therefore, at which the author arrived, was the following—viz., that instead of regarding all parts of a plant as modifications of an organ so well known and so strongly marked by its external character as a leaf, they must be regarded as modifications of a simpler internal structure or typical form, which he designated as the Phytotype, and the law of morphology will then assume the following form, "that every part of a flowering plant is a modification of an archetype, which consists of a *spiral vessel surrounded by cells*—the *spiral being simple*, or *multiplied and branching*, *and the cells being of various forms and strength*, *according to the purpose they have to serve.*

3. *Notice of Ferns from Old Calabar, sent by the Rev. W. C. Thomson to William Oliphant, Esq.* Communicated by Professor **Balfour.** The following is an extract from Mr Thomson's letter:—

"Ikoneto, Old Calabar, October 29, 1859.

"I write chiefly on account of the inclosures, and to mention that I hope you will get from Glasgow a bottle containing living specimens of the water fern, an exquisite little water moss, a Lentibulariaceous plant, and one or two other kinds. The fern is the principal specimen. It seems to be *viviparous.* I found it not many days ago, while up the Odot Creek in our neighbourhood on business. There was to be seen here and there a plant or two, floating amid sheets of the little moss near the banks, in parts where the current ran less rapidly, its aerial fronds with their narrow revolute segments rising aslant above the surface, while the natant leaves spread out their broad segments a little beneath, being submersed rather than natant. What is novel in it to me is its mode of multiplication, the species being propagated not by the usual spores, but by axillary and occasionally marginal buds becoming perfect plants on the parent frond. The pressed portions inclosed exhibit the two forms the fronds assume, on one of which the little marsupials are shown in various stages of growth. A minute roundish leaf is the first produced, others following with gradually increasing dimensions, but of the same form, till a genuine frond is unrolled, the little plant having in the meanwhile shot out several roots vertically into the water, and laterally along the surface of the parent segments. The aerial fronds of this plant are much divided into narrow segments, which have their margins rolled back upon themselves, so as to make them look narrower still. The segments of the submersed leaves are broad. The embryo plants thrive equally well with their leaves above or below water, and separation takes place only by the decay of the mother frond."

The aquatic fern referred to by Mr Thomson is *Ceratopteris thalictroides*, (Brongn.) It is found in the tropical parts of the Old and New World, and has been figured in Hooker's "Exotic Flora." Its viviparous character is well known. The specimens sent are very characteristic. An *Asplenium* sent by Mr Thomson is not easily determined. It is possibly only a form of *A. Trichomanes.* In the Grevillean Herbarium, there is a fern from New Holland very much resembling it, but unnamed. The rhizomatous fern of Mr Thomson is a *Davallia*, probably a form of *D. Canariensis.* In many respects it resembles *D. bullata*, which is found in Assam and Nepaul, as well as *D. pyxidata* of the southern hemisphere. The segments of the frond are rather broader and longer than those of the ordinary form of *D. Canariensis.*

4. *On the Palms of the Feejee Islands.* By Mr WILLIAM MILNE. Communicated by Professor BALFOUR.

Mr Milne stated that after the departure of the Herald for Sydney on 28th October 1856, he examined the palms of the Feejee Islands, and the following are those which he observed:—1. *Cocos integrifolia.* 2. Dwarf Coco-nut Palm, which seldom exceeds 12 feet in height. 3 and 4. Two species of *Areca.* He also noticed several forking varieties of palms as occurring on the islands.

Thursday, 8th March 1860.—Professor BALFOUR, V.P., in the Chair.

The following Communications were read:—

1. *Contributions to Microscopical Analysis. No.* 2. Celastrus scandens (Linn.), *with remarks on the Colouring Matter of Plants.* By GEORGE LAWSON, Ph. D., Professor of Chemistry and Natural History, Queen's College, Kingston, Canada. Communicated by Professor BALFOUR.

(This paper appears in the present No. of this Journal)

2. *On Trichotomous Arrangements of Plants.* By Mr WILLIAM MITCHELL, Associate, Botanical Society.

Mr Mitchell remarked that in the classification of natural objects, of which our knowledge is limited, it is desirable to have the divisions capable of embracing all that the progress of discovery may make known. This advantage, however, we cannot easily gain unless it is in our power to make use of the number, position, presence or absence, of parts or organs as essential characteristics in our comparison of the objects in question. But on glancing over the more common classifications of plants, we find cases admirably illustrative of scientific tact in seizing hold of such leading characters as render the divisions all-embracing in their own provinces; and singularly enough these divisions generally come out in threes. A principle of arrangement was thus struck out, which was partially employed by Ray; but clearly enunciated by Jussieu, when he ushered in that grand and ever-memorable ternary of the *Dicotyledonous*, *Monocotyledonous*, and *Acotyledonous* plants. He could now pass at once to the flowering plants, and produce another trichotomus arrangement in the same manner; which he did in his *Polypetalæ*, *Monopetalæ*, and *Apetalæ.* Mr Mitchell then proceeded to illustrate his remarks by various instances of trichotomy in the vegetable kingdom.

3. *On the Indian Woods that have been tried for Engraving.* By ALEXANDER HUNTER, M.D., Madras.

It may interest the public to know that the results of the experiments commenced in the School of Arts at Madras in 1858, to improve the illustration of the literature of India, attracted considerable attention in London, Edinburgh, and Glasgow, where specimens of the woods and of the engravings upon them were exhibited. For several years the attempts were very feeble and indifferent, although much on a par with the early efforts at illustration in England about sixty years ago. Rewards were offered for the best kinds of wood produced, and the following were the results:—

The Guava (*Psidium pyriferum*) was found to be close-grained and moderately hard, with a thin bark and pretty uniform texture of both the outer and inner parts of the wood when cut across the grain. It cut easily and cleanly like firm cheese, and gave delicate lines; but being a little softer than boxwood it did not stand the pressure of printing, though it yielded very good impressions with a burnisher. The art of printing from woodcuts being in its infancy for illustrating literature in India, many of the early impressions were spoilt from too heavy pressure. For four or five years the guava was used, and answered well for bold engraving, or for cutting blocks for large letters; attempts to cut small letters upon it for a Tamil alphabet proved a failure, though the large Tamil and English alphabets succeeded very well, and were useful for several purposes, as printing large school and diagram letters, stamping on cloth and clay to get letters or numbers for use in schools. The guava-wood was found to vary very much in texture, the large trees yielding a soft, coarse wood, while the small wood from hilly districts was hard and fine in the grain. Samples that had been sent to England, and tried for engraving, were pronounced to be too soft, and inferior to English boxwood.

The Satinwood of Ceylon (*Chloroxylon Swietenia*) proved to be hard, but uneven in the grain, coarse in the pores, and, like many woods of a large size, harder and denser in the centre than near the bark. Under the graver it was found to splinter, and not to cut sweetly or turn over in curls as it ought to do. This wood was condemned as unsuited for wood-engraving both in Madras and England.

The Palay (*Wrightia tinctoria*). The native name is a very vague one, being applied to a number of woods that have a milky juice. The wood, however, is better known to the public as one from which native toys are frequently turned. It is a pale, nearly white wood, close and uniform in the grain, but too soft to stand printing. It cuts smoothly, but does not bear delicate cross-hatching. It was pronounced unfit for wood-engraving in England, though well suited for turning, carving, and inlaying with darker woods. A kind of indigo is *obtained* from the leaves of this tree.

Veppaley or *Wrightia antidysenterica*, was found to be very hard in the centre, but soft in the outer portions, and liable to the attacks of insects. On examining this wood under the microscope it gave promise of being suitable for the purpose, from the closeness of texture and the polish left by the chisel in cutting it across the grain, but the uneven quality and the softness of the outer parts showed that it was not fit for engraving. Its chief use is for posts and rice-beaters.

Sandalwood (*Santalum album*) proved to be the nearest approach to the boxwood in working quality, hardness, and durability under pressure. This is a moderately-sized wood, with thin bark, which is usually a criterion of fine even grain. It cuts smoothly, the chips curl well under the graver, and the oily nature of the wood seems to preserve it from splitting when wet. There are considerable differences in sandalwood, according to the locality from which it is procured, the small, dark coloured wood of 5 inches diameter, grown on dry rocky soil, being the best. Many hundred engravings have been executed upon this wood, and it has been found occasionally to equal boxwood, though it is not quite so hard. It is an elastic wood that hardens on exposure to the air, and stands a good deal of rough usage in the press; some blocks have yielded upwards of 20,000 impressions without being worn out. The large pale saudalwood is not so good as the small dark kinds. This wood was not tried in England, as its price was thought to be too dear, but on comparing it with boxwood, which sells in England for one penny the square inch, it was found to be cheaper in India than boxwood in England, though it is ten or twelve times the price of any of the other woods that were tried.

The Beyr-fruit tree (*Zizyphus Jujuba*) gave good promise under the microscope, but proved to be a soft, spongy, light wood, that did not stand cross-hatching or pressure. It is used for native sandals.

The wood of the wild orange (*Citrus Aurantium*) bears a strong resemblance in appearance to box in working qualities, and is often as hard, but, like the sandalwood, the small old trees from the hilly districts yield the best wood for engraving. It has a very thin bark, a bright yellow colour, and a very uniform and close texture. The cultivated or garden orange has a coarse wood with a very uneven texture, produced in some cases by a curious mode of propagating the trees—viz., by splitting down the parent stem and planting every piece that has a root attached; a barbarous and primitive mode of culture, but thought by the natives to improve the fruit.

It was reported that a kind of boxwood was common in the gardens about Madras, but on procuring a specimen of the flower and fruit of the tree for examination, it proved to be a species of China orange, the *Murraya exotica*, with a very small fragrant fruit little larger than a pea. On trying the wood for engraving, it proved to be like the wood of many of the Aurantiaceæ or orange family, hard and close in the grain near the centre, but softer near the bark. The cross section of this tree is very irregular, being deeply indented, from the same mode of propagation as is followed with some of the garden orange trees. The result of this is that both the wood and bark of the tree are impaired, though the flowers and fruit are not. The flower of this plant is used by bride's-maids instead of the true orange blossom, which it resembles.

A wood that disappointed the expectations that had been formed of it from the first trial was coffee (*Coffea arabica*). The first piece of this that was sent to the School of Arts was very hard, uniform and close in the grain, but small. Some pieces of old trees, about 6 inches in diameter, were afterwards procured, but they proved to be soft, uneven in grain, and not fit for engraving, though the wood is well adapted for ornamental carving or inlaying. We should be glad to hear more about this wood, and to receive other specimens of young and old wood cut when fresh. The specimens sent us were old trees that were past bearing, and that had been pulled up, left on the ground for a few weeks, and then dried near the cook-room fire for some days; a great mistake, as woods for engraving ought not to be too dry. This wood works beautifully on the turner's lathe, and cuts very sharply under the chisel, gouge, or graver; it is deserving of more attention for ornamental carving and inlaying. It harmonises well in colour with the orange and with the wood of the *Inga dulcis* or Corookapoolee. It approaches in colour and grain to walnut, but is too coarse for engraving, though fit for gunstocks and cabinet work.

The only other woods tried for engraving were a *very* close-grained fine and uniform wood which was sent from the Neilgherries under the name of iron wood, used for turning and for making walking-sticks. It worked well under the graver and on the turning lathe, but the piece sent was too small to print from. And a piece of *Fustic* (*Maclura tinctoria*) that had been grown in the Horticultural Gardens at Madras, but this proved to be too soft and coarse for engraving, though a rich-coloured bright yellow wood, suited for inlaying.

About two years ago, it was reported that true boxwood was discovered in the North-West Provinces, and a log of it was kindly procured for the Madras School of Arts by Captain Maclagan, of the Roorkee College, and forwarded to Calcutta for despatch to Madras; but it seems to have been appropriated for use in the School of Arts in Calcutta, where a prize of 500 rupees was offered for the best substitute for English boxwood fit for engraving. We do not yet know if the prize has been awarded, but we heard from a friend who had lately visited the School of Arts in Madras and Calcutta, that a good deal of boxwood has been sent to the latter school, and our log is one of those probably. We should think the prize of 500 rupees too large for such a discovery in Madras, as we have collected all the above-named woods, and used some of them for engraving and illustrating scientific and educational books, reports, and many of the advertisements in the Madras newspapers, and all have been the result of a reward of 10 rupees offered to Captain Puckle who sent us the best collection of woods, and who liberally handed over the reward to the natives who collected the specimens. We have to deplore the loss of the services of our best wood-engravers in Madras. Mr Garrick, who was at the head of this department, has been tempted away from us to Calcutta with the offer of a high salary. Mr J. Duarte, Mr Sharleib, and many others who used to render us valuable aid, have obtained more remunerative employment for their talents than we could afford to give, and we are reduced to one intelligent deaf and dumb native lad, who promises as well as any of the above named.

We have still got a good staff of engravers and etchers upon copper. This style of work was pointed out to us in England as one in which the natives of India were calculated to excel, as it admits of a free and flowing kind of line which cannot be easily imitated in wood engraving. We wish our former teachers and pupils every success in after life, and should be glad to see them trying to aid in the extension of a taste for the fine arts, or for illustrating literature in Southern India. A good many lads began to learn wood engraving in Madras, but few of them had the perseverance to carry it on, chiefly, I believe, on account of its difficulty, and the time and labour required to be expended on its study.

4. *On Disease of the Nutmeg Trees in Singapore.* By Mr ANDREW T. JAFFREY.

Mr Jaffrey has recently examined the nutmeg plantations of Penang and Singapore, and states that "there is a real possibility of ruin staring the proprietors of the plantations in the face, and the chances are that, unless remedial measures are adopted to arrest the present deterioration of the trees, which is almost universal, there is a probability that there will be the extinction of a valuable article of commerce. It was perfectly evident, when visiting the islands a few months ago, that some fatal malady had seized upon the trees. The cause of this effect may be difficult to discover, but, judging from appearances only, the conclusion come to was that the disease was *local*, and not *constitutional;* therefore there was a hope that it would be overcome." There was a yellow sickly appearance of the foliage, the branches here and there showed symptoms of decay, and the fruit dropped off before ripe. Mr Jaffrey considered that the disease arose in a great measure from the system of using green manure, which caused the development of fungi on the roots of the plants.

Mr Burnett exhibited branches of hollies gathered at Killarney, exhibiting great variation in the forms of the leaves. The specimens were so different that they might have been regarded as distinct varieties, and yet most of the variations occurred on the same tree.

Thursday, 12th April 1860.—Professor BALFOUR, V.P., in the Chair.

The following Communications were read:—

1. *Notes on Californian Trees.* By ANDREW MURRAY, Esq., F.R.S.E. Part III.

This was a continuation of the papers on Californian trees, two of which have been already published. The most interesting trees noticed on this occasion were *Pinus Lambertiana,* an enormous pine of the Weymouth section, growing 200 feet in height, and bearing long pendant cones from 1 to 2 feet in length. It was first discovered by Douglass, who gives an interesting account of the occasion on which he met with it. He was in the neighbourhood of hostile Indians; but, notwithstanding the risk of bringing them down upon him by the sound of firing, he could not pass the magnificent cones without making an effort to procure them. He shot down a couple, but, the Indians immediately appearing, he had to scamper off, thankful that he had got even these. From some beautiful photographs of Californian scenery which were shown by Mr Murray, in which this tree is recognisable, it appears to form a most striking object in the landscape, stretching its arms far out horizontally over the tops of smaller trees. It is quite hardy in this country, and is now pretty generally introduced. Its timber is one of the most valuable in California. It is more accessible than *Pinus Monticola* or *Pinus Murrayana*, which are most used when they can be reached, and has the advantage of growing to a great size, with a straight boll 60 to 100 feet without a branch, and a great deal of timber is thus got from a single tree. This tree distils a peculiar resin, which is sweet to the taste, and has caused the tree to be known in California by the name of the sugar pine. Certainly a fir tree appears the last thing from which one would dream of extracting sugar, but Professor Lyon Playfair has had the kindness to analyse the sugar sent from this tree, and finds that it is in all respects the same as the sugar of the sugar-cane. *Pinus Monticola* is another beautiful tree of the Weymouth class, which is now so well known that a description is unnecessary. The wood is magnificent timber; nothing can be better. It grows at great elevations, being the last timber on the tops of the mountains, and the greater the elevation the bigger it grows, attaining 4 feet in diameter. A very fine example has produced cones in the Keillor *Pinetum* in Perthshire for several years past

Abies grandis.—Very considerable doubt is felt as to which is the true grandis, horticulturists having got three or four young plants very near each other, which have given rise to much uncertainty. There is the true *grandis*, the *amabilis*, the *lasiocarpa,* and another with a well-marked cone, sent home by Jeffrey, but apparently not described. We find all these in their turns bearing the name of *grandis*, and it will be some time before we can satisfactorily allot the young plants to the typical species originally described under the above names. They are all lofty trees, 150 to 200 or even 280 feet in height, but the quality of the wood seems doubtful. Speaking of *amabilis*, Mr Murray mentioned that his brother says that it is a coarse and useless wood. The species brought by Jeffrey appearing to Mr Murray to be distinct, he described it under the name of *Picea campylocarpa*. The cone is larger and much longer than that of *A. grandis* (about seven inches in length), and usually distinguished by a bend or elbow in the middle (whence the specific name). The leaves are short like those of *P. nobilis*, and have the same curl which is the character of the leaves of that pine. A large importation of seed of this pine has been reared by Messrs Low of Clapton, who have issued them as *P. amabilis*.

Cupressus Lawsoniana (Murr.)—A coloured drawing of this most beautiful of all the cypresses, taken by Mr Peebles on its native soil, was exhibited by Mr Murray. It is figured as standing on the edge of a waterfall, from which a hint may be taken as to the best position in which to plant it. It is quite hardy, and a plant was exhibited by Mr M'Nab, bearing fruit, although it was only introduced in 1854.

Taxus Lindleyana (Murr.)—A figure of this yew was exhibited. It showed a wide-spreading open-branched tree, and anything more unlike our preconceived notions of the characters of a yew cannot be imagined. The wood is very hard, and has been successfully made use of for wood-cutting. *Juniperus Californicus* has also been tried for this purpose, but was found not suited for anything but coarse work. It grows to a considerable height (forty or fifty feet), and has an umbrella top, which contrasts well with the spire-topped trees, which are a marked feature in Californian scenery.

2. *Remarks on the Vine Disease as it has been observed at the Cape of Good Hope.* By S. J. Meintjes, Esq.

The author gave an account of the ravages of this disease among the vines at the Cape, and stated that it had been traced to the presence of the fungus called *Oidium Tuckeri*, drawings of which he exhibited. The disease had been referred to a long continuance of damp weather. The chief remedy employed was sulphur, either alone or in combination with lime.

3. *On* Sarcina Ventriculi *of Goodsir.* By John Lowe, M.D., Lynn.

(This paper appears in the present number of this Journal).

4. *On the Poison Oak of California.* By Dr Colbert A. Caulfield, Monterey. Communicated by Andrew Murray, Esq.

The "Poison Oak" is one of the great plagues of California. The plant is widely diffused, and numerous cases are constantly occurring in every district of persons suffering severely from its effects. Many antidotes and remedies have been published, though still there is a demand for more information on the subject. In the woods and thickets of California, as well as on the dry hill-sides, and in fact in every variety of locality, may be found a very poisonous shrub—the "poison oak" or "poison ivy," the *hiedra* of the Spanish people. The plant belongs to the natural order *Anacardiaceæ*, and is *Rhus varielobata* (Steud.) or *R. lobata* (Hook). It is very similar to the poison ivy of the Atlantic States, *R. Toxicodendron* (Linn.), both in its appearance and its poisonous qualities. This poison is the cause of a vast deal of misery and suffering in California, and there is scarcely ever a time in any little town or neighbourhood where there are not one or more persons suffering from cutaneous disease in consequence of coming in contact with the plant. The remedies in use for the effects of the poison oak are various, and some of them will cure the milder cases. Of all the common remedies, the warm solution of the sugar of lead has, within my experience, been productive of the best results. The water of ammonia, warm vinegar and water, the warm decoction of the leaves of *Rhamnus oleifolius* ("Yerba del oso" of the Californian Spanish), or even pure warm water, are sufficient sometimes to produce a cure. All these remedies are, of course, applied externally by way of washes to the parts affected. But the only remedy that I have found invariably successful as an antidote for this poison is an indigenous plant growing very abundantly in this vicinity (Monterey), and in other parts of the State. It is a tall, stout perennial, belongs to the composite family, and looks like a small sunflower. It is from one to three feet high, has bright yellow flowers in heads one or two inches in diameter, and, as I have said, like small sunflowers flowering from June to October. Before flowering, the unexpanded heads of buds secrete a quantity of resinous matter. The whole plant, when growing on dry hills, is stiff and rigid, with narrow thin leaves; but in damp localities it is more robust and succulent, with wide fleshy leaves. Its botanical name is *Grindelia hirsutula* (Hook. and Arn.) The mode of using it is as follows :—The fresh herb may be bruised and applied by rubbing it over the parts affected; or, boiling it in a covered vessel, make a strong decoction of the fresh or dried herb, with which to wash the poisoned surfaces. Its remedial properties appear to be contained chiefly in the resin or balsam-like juice of the plant, which is particularly abundant on the surface. One application is sometimes sufficient for a cure; but if the disease has been of long duration, several days will elapse before relief is obtained. This plant is a remedy for the poison oak, used originally by the Indians of this vicinity, and by them its virtues have been communicated to the Spanish-Californian people, who are now commencing to use it. It may not be amiss to say, in conclusion, that the *Grindelia* is used also by the people of the country as a remedy for other cutaneous diseases which are characterised by heat and itching, such as nettlerash.

Sir William Jardine, Bart. stated to the meeting that the winter had proved very severe on the plants at Jardine Hall, which is 260 feet above the sea, and about 11 miles from it as the crow flies. He had lost *Buddleia globosa*, *Weigelia rosea*, *Leycesteria formosa*, and many other similar plants. *Cupressus M'Nabiana*, and *Larix Griffithii* had been destroyed, and the small tips and shoots of *Abies taxifolia* have been injured. *Taxodium* has not suffered. The other plants from Oregon have been untouched, and none of them were protected. *Abies Pattoniana* is strong and hardy.

Thursday, 10*th May* 1860.—Professor Allman, President, in the Chair.

The following Communications were read :—

1. *On Some Peculiarities in the Stem of the Ivy.* By G. Ogilvie, M.D., Aberdeen.

The points specified in the paper were principally the following :—The complete fusion of the stems of the ivy, when strongly pressed together in the course of growth—the structure of the aerial rootlets or claspers—the abundant deposit of starch in the woody fibres. The fusion was shown to be at times of the most intimate nature. All the layers of bark are continued in a uniform sheet over the line of junction, and can be freely peeled off both stems at once, indicating that the cambium layer passes uninterruptedly from the one to the other; and the woody cores appeared in many cases to be as closely compacted as if they had been a single stem. It was observed that when the axes nearly coincided the outer layers of wood formed a continuous envelope over the two stems, but when they were very divergent in direction, the woody tissue was confused at the line of junction, the fibres abutting irregularly against each other. Maceration had no effect in separating the concrete stems, nor had desiccation in many cases, though occasionally cracks formed along the lines of junction from each stem shrinking to its own centre. Such fusion contrasts strikingly with what occurs in other woods, when stems are anyhow compacted together by the force of growth. The bark in such cases never peels clean off over the line of junction, but insinuates itself between the stems in a morbid and degenerate condition; nor is the woody tissue fused, but injured and stopped in its growth at the point of pressure, in one or both stems. Twisted osier stems perhaps make the nearest approach to such fusion, but the union of substance is always imperfect, as becomes very apparent on drying. In regard to the claspers, it was stated that their structure, though undeveloped, was essentially that of rootlets, and that they originate in the same way in the cortical layer—pushing outwards, on the one hand, a free extremity, and on the other, connecting themselves with the woody tissue, generally at the emergence of a medullary ray, the central cells of which appear to become condensed into a fibrous prolongation inwards of the rootlets. The occurrence of starch granules in the wood-cells was noticed as a character which, though not unknown in other cases, might still be regarded as an exceptional arrangement, to the extent at least to which it prevails in the ivy, where the deposit is most abundant, in the woody fibres both of the alburnum and duramen.

The paper was illustrated by microscopical preparations and drawings, and by specimens of stems which had coalesced in the way described.

2. *On the Effects of Irritant and Narcotic Gases on Plants.* By JOHN S. LIVINGSTON, Esq.

(This paper appears in the present number of this Journal).

3. *On the Poisonous Qualities of* Lathyrus sativus *in India.* By Dr GEORGE BUIST, Allahabad.

Dr Buist observed that the *Lathyrus sativus* had caused extensive poisoning among the inhabitants of Allahabad. The use of its seeds as food appeared to give rise to a severe form of paralysis, which in many cases proved fatal.

Mr Giles Munby, from Algiers, stated to the meeting that this plant was used extensively for food by the inhabitants both of the south of Europe and of the north of Africa, and that he had never seen any bad consequences from its use.

4. *On the Effects of the late Severe Winter on Vegetation in the Edinburgh Botanic Garden.* By Mr JAMES M'NAB.

During the past winter, considerable damage has been sustained by some of the plants cultivated in the Botanic Garden. In many places roots were frozen up for four months. We need not, therefore, be surprised if the full extent of the mischief done will not be thoroughly ascertained before midsummer, particularly with deciduous trees and perennial herbaceous plants; while with evergreen trees and shrubs every day seems to tell more and more upon them.

Several species of coniferæ have suffered where the soil is heavy and damp, while other specimens of the same species growing in dry soil have suffered less. Those species which are totally killed, and which were growing in heavy, damp soil, are the *Pinus muricata, Dacrydium Franklinii, Cupressus Knightii*, and *Cupressus Goveniana.* With the exception of the Dacrydium, all the others have stood untouched for four or five years. Of those partially injured, the most conspicuous at the present time are *Taxodium sempervirens* (particularly in damp soil), *Cupressus macrocarpa, C. funebris, Cryptomeria japonica* (only in damp soil), *Pinus insignis, P. radiata, P. Edgariana, P. flexilis, Saxegothæa conspicua, Fitzroya patagonica*, also *Larix Kæmpferi*, and *L. Griffithii.* Of the recently introduced coniferæ which have stood uninjured during the past winter, both in heavy and light soils, are *Pinus tuberculata* (Jeffrey's variety), *P. Craigana, P. Jeffreyii, P. Balfouriana, P. Murrayàna, Picea Nordmanniana, P. grandis, P. amabilis, P. lasiocarpa, Abies Mertensiana, A. Pattoniana, A. Hookeriana, A. Cilicica, Wellingtonia gigantea, Cupressus Lawsoniana, C. M'Nabiana, C. Lambertiana, Thuja gigantea, T. Craigana, Libocedrus chilensis*, and *Thujopsis borealis.* With deciduous trees, little will be observable till the full foliage season arrives. With evergreen shrubs the chief damage is amongst the Rhododendrons, most of the Bhotan varieties and many of the Sikkim sorts having fallen victims. Large plants of *Rhododendron lancifolium* are completely killed, as well as many plants of *R. ciliatum* and *R. glaucum.* Many of the hybrid rhododendrons are seriously injured, particularly the sorts raised by crossing *R. campanulatum* with *R. arboreum;* also those between *R. maximum* and *R. arboreum;* likewise between *R. ponticum* and *R. arboreum;* while the varieties raised between *R. Catawbiense* and *R. arboreum*, as well as those between *R. caucasicum* and *R. arboreum*, do not as yet show any symptoms of injury. If we should experience a continuance of hot suns without moisture, I fear others may yet give way. Besides rhododendrons, several Andromedas, *Garrya elliptica, Erica australis, E. arborea*, and *E. mediterranea*, have been considerably damaged. *Phormium tenax* is very much injured, and several plants of *Yucca gloriosa*, with stems two and three feet high, and fifteen inches in circumference, were laid prostrate during the winter. Several of these, however, assumed their upright position when the frost disappeared, while others are still procumbent, and require support, in order to keep their heads off the ground. Wall exotics have also been much cut down, particularly the New Holland species of the genera *Acacia, Eucalyptus, Bossiæa, Indigofera, Sida, Metrosideros*, and *Leptospermum;* also *Aloysia citriodora*, myrtles, and double wallflower. Herbaceous plants having roots more or less fleshy are considerably damaged, as *Momordica Elaterium, Exogonium Purga, Phytolacca decandra, Anemone patens*, and *A. alpina.* The Pampas grass appears to be totally killed in damp soil, while plants of it growing in dry situations seem as yet uninjured. *Tritoma Uvaria* is also killed in damp soil, and uninjured in dry; while *T. Burchellii* has suffered in both. Amongst biennal plants, stocks, antirrhinums, *Linaria biennis, Erysimum Perofskianum, Onopordon Acanthium, Carduus marianus*, and *C. eriophorus* have been killed. Beds of *Verònica Andersonii, Francoa ramosa*, and *Vittadinia triloba*, which have stood uninjured for several years, have likewise perished.

Mr M'Nab laid on the table flowering plants cultivated under the respective names of *Orobus flaccidus, O. vernus, O. elegans, O. venosus*, and

O. cyanus. Also a series of seedlings, raised from each of these so-called species or varieties. Many of the seedlings are similar to each other, a few only of some of the varieties partaking of the parent type.

The seedlings raised from *Orobus flaccidus* during the year 1857, although similar to each other, differ widely from the parent plant, each having linear leaves, with stems somewhat procumbent, and producing small pale flowers. Those raised during 1858 are all narrow-leaved, more or less procumbent, producing also small pale blossoms, and flowering freer than the seedlings raised during 1857. This, however, may be accounted for by their being confined in small pots. Of the seedlings raised during 1859, although most of them have narrow leaves, a few show a tendency to the typical form. One peculiarity in these seedlings is, that they flower fully one month later than the plants which produced the seed. The seedlings now shown in flower were forced for the purpose of exhibiting them along with the parent plants. Seedlings raised from *Orobus vernus* during 1858 have not yet flowered; all have narrow leaves, instead of broad, which is the character of the parent plants.

Seedlings of *Orobus elegans* raised during 1858 are all narrow-leaved and somewhat procumbent. The original plants of this variety have also a tendency to vary, but only in the breadth of leaves, as shown by the plants exhibited. Seedlings of *Orobus venosus* raised during 1858 present a more varied tendency, the proportion with broad leaves being about one in eight. Seedlings of *Orobus cyaneus* raised during 1858 are nearly equally divided between the broad and narrow leaved varieties. Mr M'Nab stated that his intention had been mostly directed to the seedlings of *Orobus flaccidus*, being one of the finest of the spring flowering varieties. In order to prevent impregnation with other species, the plants selected for the seeds have been grown for many years in front of the hot-houses in the Botanic Garden, at a distance from the general collection of *Leguminosæ*. All the parent varieties here enumerated are upright growing, and all early flowering, and therefore past before the other species come into flower, so no impregnation with inferior varieties could possibly take place. Some tendency to vary had also been observed amongst seedlings raised from some of the late flowering species of *Orobus*, also with seedlings raised from the genera *Vicia* and *Lathyrus*, but these have not yet been tested.

Mr M'Nab exhibited growing plants of *Lastrea dilatata* having the stipes of each frond branched, and each division producing perfect fronds. He likewise exhibited seedlings raised from these plants during the spring of 1859, all presenting the same branching appearance, and although the fronds are not yet three inches in length, they are covered with perfect sori.

PUBLICATIONS RECEIVED.

Proceedings of the Manchester Literary and Philosophical Society. (Continued.)—*From the Society.*

Journal of the Asiatic Society of Bengal, Nos. 274, 275.—*From the Editor.*

Quarterly Journal of the Chemical Society, No. 48.—*From the Society.*

L'Institut. (Continued.)—*From the Editor.*

Answer to Hugh Miller, &c. By Thomas A. Davies.—*From the Publisher.*

Canadian Naturalist and Geologist, June to December 1859, and February to April 1860.—*From the Editors.*

The Atlantis, No. 5, January 1860.—*From the Editor.*

Chronica Regum Manniæ et Insularum. By Professor Munch of Christiana.—*From the University of Christiana.*

Ueber die Geometrische Repräsentation der Gleichengen Zurschen zwei Veranderlichen reellen oder Komplexen Grossen. Von C. A. Bjerknes.—*From the same.*

Jagttagelser over den Post-pliocene eller Glaciale Formation den del af deh sudlige Norge. Af Prof. Sars und Lector Kjerulf.—*From the same.*

Observations sur les Phenomènes d'Erosion en Norvège. Par J. C. Horbye.—*From the same.*

Memoirs of the Geological Survey of India, Vol. I. Part I.—*From the Government of India.*

Jahrbuch der Kaiserlich Konigliche Geologischen Reichsanstalt. July, August, and September 1859.—*From the Society.*

Verhandlungen der Naturhistorischen Vereins der Preussischen Rheinlande und Westphalens. Von Prof. C. O. Weber. Bonn, 1859. 1-4 Heft.—*From the Society.*

Annual Report of the Superintendent of the Geological Survey of India.—*From the Superintendent.*

Proceedings of the Academy of Natural Sciences of Philadelphia, 1860. —*From the Academy.*

Journal of the Academy of Natural Sciences of Philadelphia, New Series, Vol. IV., Parts I. and III.—*From the Academy.*

Natural History Review, April 1860.—*From the Editors.*

THE EDINBURGH NEW PHILOSOPHICAL JOURNAL.

On the Elevation Theory of Volcanoes, in Reply to a Paper of Mr Paulett Scrope, read before the Geological Society, Feb. 2, 1859; *being the Substance of a Communication made to Section C., at the Meeting of the British Association for the Advancement of Science, held at Oxford in* 1860. By CHARLES DAUBENY, M.D., L.L.D., Professor of Chemistry in the University of Oxford.

When Sir Charles Lyell, in the able Memoir he published in the "Philosophical Transactions" for 1858, had exposed, by a train of carefully conducted observations, the fallacy of M. Elie de Beaumont's position, that sheets of compact lava could have been found only upon gentle slopes, I, for one, was thankful to him for being enabled to extend to all that portion of Vesuvius which falls under our review the same mode of formation which we see illustrated in the more recent of its beds produced within our own memory. Whatever may be the case with regard to the nucleus of the mountain which lies concealed from our sight by innumerable sheets of superimposed lava, one was naturally glad to fall back upon the simpler notion as to the building up of a volcanic mountain by the successive outbursts of beds of lava and showers of scoria, which the older geologists had espoused, so soon as it had been shown that the high inclination which many of these beds assume constituted in fact no valid objection to this mode of explaining their origin; and although nothing can be more illogical than to regard the removal of an objection to an hypothesis as in itself tantamount to a proof of its validity, I cannot wonder that the establishment of the fact, that lavas have been consolidated at high angles, should contribute to give a preponderance in the eyes of geologists to the eruption theory, and to bring into comparative discredit the opposite one of elevation.

So much, indeed, has this been the case, that it seems to be assumed by many in the present day, that the latter theory is as completely put out of court by the late researches of Sir Charles Lyell, as the opposite one was supposed to be about ten years ago by the investigations of M. Elie de Beaumont; at which period, Mons. Duperry, with a full knowledge of the current opinions of the geologist, would take upon himself to affirm, that although formerly it had been imagined that a volcano was built up by successive eruptions of lava and ejections of ashes, no one at that day would venture to maintain so extravagant a position.

To prevent these and similar revolutions of opinion from being quoted against geologists as proof of the unstable foundation of their theories, it might be well to bear in mind, that although it may be perfectly legitimate to extend, so far as we are able, to volcanic rocks in general that mode of formation which we see going on before our eyes in volcanoes now in activity, and to abstain from going farther for an explanation, until we have satisfied ourselves that the former is inapplicable to the particular circumstances of the case we are contemplating, yet that, considering the great variety of circumstances under which volcanoes have been formed, and the numerous phases of action they display in their operations past and present, it would seem hasty and presumptuous to assume that the mode of formation which we witness in a few familiar cases should be applicable to all the remainder.

It is on this account that I feel myself called upon to notice the Memoir of Mr Paulett Scrope, published in the "Quarterly Journal of the Geological Society" for November 1859, which—but for the too dogmatic tone it adopts, and the confidence with which it treats as exploded fallacies the opinions of such men as Humboldt, Von Buch, Duperry, and the like,—I should have hailed as presenting an useful summary of the arguments that may be alleged against the elevation theory, as well as a warning against following too implicitly the guidance of the French school of geologists in their interpretation of the phenomena of volcanoes.

But Mr Scrope does not rest satisfied with this more practical and less ambitious aim, but would wish to persuade us that no such event as the sudden rise of a volcanic, or, I presume, by parity of reasoning, of any other mountain whatever, has or ever can have occurred; thus placing me—as the author of a general work on the subject of Volcanoes—under the alternative, either of abandoning as unsound the arguments upon which I therein maintained the general principle of elevation, as applicable to the case of volcanoes, or of attempting to show, that they remain in a great degree unshaken by his reasoning.

Perhaps, indeed, the same remark may apply to my speculations, which Mr Scrope has himself suggested, to account for the want of attention paid to his own remarks by those great authorities who still, in defiance of them, continued to the end of their days to maintain the elevation theory; and I may not be wrong in supposing, that in pursuit of higher game he has overlooked the arguments advanced in my more humble volume.

Still, as he has done me the honour of coupling my name with those of M. Elie de Beaumont, Baron Humboldt, Von Buch, and Professor James Forbes, as amongst the number of those who have advocated, at least to some extent, the elevation theory, it might have been expected, that in so elaborate a paper he should have touched upon the principal arguments in favour of that view contained in my work on "Volcanoes." On the contrary, it would appear as if Mr Scrope imagined, that the only direct proof ever offered of the elevation of a volcano was the celebrated case of Jorullo; for, with respect to that of Methone in Argolis, alluded to by Ovid, he dismisses it as a poetic myth, forgetting that the fact of the elevation of this promontory is vouched for by many prose writers of antiquity, and especially by the accurate Strabo, whilst it is countenanced by the observations of moderns who had visited the spot, as by Voilet and Boplarge, in their account of the French expedition to the Morea.

But passing this over, let us proceed to the celebrated case of Jorullo, where we are told that a tract of ground, from three to four square miles in extent, became elevated 524 feet above its former level; whilst, from the midst of the swollen or upheaved mass, six conical hills, varying in height from 300 to 1600 feet, appeared in the course of a single night, rising above the original level of the plain.

This phenomenon, which Baron Humboldt, after an inspection of the spot, pronounced to be owing to an upheavement of the country by subterraneous agency, Mr Scrope, without having visited the locality, undertakes to account for by the mere pouring out of lava from a volcano, formed, like the Monte Nuovo, by a sudden outbreak of volcanic energy.

He remarks, that some of the lava streams of Iceland equal in thickness the general upheaval at Jorullo; alluding, no doubt, to the great eruption of Skaptaa Jökull in 1783, but overlooking the fact, that the stream in this instance, when it attained that depth, was pent up within a narrow gorge, on emerging from which, and arriving at a country where it had liberty to expand, its depth never exceeded 100 feet.

He also maintains, that if the lava was only imperfectly fluid, and if the surface over which it flowed had been quite level, there is no reason why it might not have been circumscribed within the limited area which it actually covers.

But the difficulty which presents itself to my mind in accepting such a solution, is that of conceiving the possibility of any mass of matter, sufficiently approaching to fluidity to have descended with such rapidity the steep incline of the central conical hill—to which Mr Scrope traces it—being arrested in its downward course at so short a distance from its point of issue as to have merely mantled round the base of the volcano in the manner which Humboldt represents.

It strikes me that, if the lava were fluid enough to have flowed down the sides of the mountain, and to have reached its base in the short time recorded, it would have acquired an impetus which would have propelled it onwards, in one direction or another, for a considerable distance, so that the limited area it occupies at the foot of the mountain is to me the best evidence of its not having descended as a current from the eminence to which Mr Scrope traces it. Mr Scrope, indeed, cites as a parallel case, on the authority of Pestal and Lenz, the great volcanoes of Awatscha in Kamtschatka. This mountain, it seems, emitted a stream of lava, which, when it reached its base, was so speedily arrested in its progress, as to form a sort of promontory of considerable height, jutting out from the flank of the volcano.

But the parallel fails in two respects—*first*, because we do not know what period of time had been occupied in the piling up of this mass of lava; and *secondly*, because the latter is confined to a single spot, and does not march round the base of the mountain, as is the case with Jorullo. Without, indeed, adopting those peculiar views with regard to the fluidity of lava which Mr Scrope ventured upon in his earlier publications, I can readily admit that, like glass or pig-iron, the products of volcanic operations may exist in every degree of viscidity, from a state not far removed from the solid, to one admitting of a free motion of the particles; and hence I can the more readily understand the upheaval of large masses of ignigenous materials in a pasty or semifluid condition. But I cannot so well imagine a lava-current, fluid enough to descend rapidly the slope of a mountain, being arrested suddenly at its base, or covering the level ground immediately encompassing the latter, without invading the territory beyond.

In the present state of our information, therefore, most geologists, I believe, would be disposed to accept the original hypothesis of Humboldt as less open to objection than the one which Mr Scrope has proposed in its place; and although it is possible that subsequent investigations may lead to a different interpretation of the phenomena, yet they must proceed from persons who have visited the spot, and not from speculators at a distance.

I shall be glad, therefore, to learn the results of the journey which it appears M. de Saussure has recently made to the locality. He is said by Mr Scrope to have convinced himself of the erroneousness of Humboldt's theory; but before he can expect us to adopt his conclusions, he must be prepared to show, either that the facts which the Prussian philosopher has recorded with respect to this volcano are themselves untrue, or that they are reconcilable with the march of an ordinary eruption.

Humboldt, indeed, was not an eye-witness of the eruption he describes; but the sudden elevation of a tract of land is an event to which the inhabitants of the neighbourhood, upon whose authority he records it, would have been as competent to bear testimony as the most scientific observer.

Now, that which gives the peculiar significance to the case of Jorullo is its affording a key to the formation of those numerous volcanoes which have from time to time been elevated in the midst of a deep sea, to some of which, I may remark, Mr Scrope has alluded.

Of islands raised by elevation, I may enumerate the following, as having occurred within historical times:—

1. The rock which Langsdorff describes near the island of Unalaschka, in the Aleutian group, 3000 feet in height, consisting of trachyte, which made its appearance in 1793.

2. The island of Sabrina, near St Michael's, in the Azores, about a mile in circumference, and from 200 to 300 feet above the level of the ocean, which rose suddenly in the midst of the sea, and after continuing in sight for some weeks, again disappeared.

3. The island of Santerino and its appendages, in the Grecian Archipelago, which have been thrown up on various successive occasions, the earliest event of the kind recorded in authentic history being 197 B.C., according to Pliny and others. The next event of the kind happened A.D. 96, in the reign of Claudius; the third in 1573, when the rock of Little Commeni was thrown up; and the last, the one recorded by Father Goree in 1707, when a new island arose between the Great and the Little Commeni. Professor Forbes, the last scientific traveller who visited the spot, states his own impression to be, that these islands together constitute a crater of elevation, of the walls of which the outer ones are the remains, whilst the central group is of later origin, and consists partly of upheaved sea-bottoms and partly of emptied matter, poured forth, however, beneath the surface of the water. He further

informs us, that the shells which he collected in the bed of pumiceous conglomerate, constituting the mass of the island observed by Father Goree in the act of rising, consisted of species which could not have lived at a less depth than 220 feet below the surface of the water, thus showing the extent of elevation to which this rock had been subjected.

4. The island thrown up near Iceland in 1783, about 30 miles south-west of Cape Reykianas, which sunk again within a year after its elevation.

5. The phenomenon which occurred off the coast of Sicily in 1831, when an island 3840 feet in circumference, and rising 107 feet at its highest point above the sea, suddenly appeared on the 13th of July, and sunk again in the latter end of December. It was first visited by Captain Swinburne, who gave it the name of Graham's Island; and was afterwards explored by Dr John Davy, M. Constant Prevost, and others.

I have enumerated all the cases of sudden elevation that have occurred within the memory of man in the midst of deep water, because, although passed over by Mr Scrope, they do not seem to me easily explicable by the common hypothesis. If produced merely by an accumulation of loose masses thrown up from some submarine vent, it might be expected that their appearances would have been less sudden, and their slope more gradual.

In the instance of Graham's Island, it would appear that the soundings round the coast rapidly sunk from 1 to 40 or 50 fathoms. Had the island been built up by a gradual accumulation of loose fragments, ought not the sea for many miles round to have had its depth diminished in consequence?

Moreover, it is recorded by Dr Davy, that whilst the general temperature of the sea was at the time 80°, that immediately about the island reached no higher than 70° or 72°, a circumstance which Arago explained by supposing a mass of rock possessing a lower temperature to have been thrust up through the midst of the waters.

If we may be permitted to embrace within the question those numerous volcanic islands which appear to have been derived from outbursts of volcanic energy taking place in the ocean at periods anterior to man's observation, still greater difficulties present themselves in many cases to the application of the received theory. In the island of the Great Canary, for instance, Von Buch describes the nucleus of the crater as consisting of trachyte, which therefore must have risen from the bottom of a deep sea to the height of some thousand feet above its surface. The same is represented to be the case in the contiguous island of Palma, where from the summit of the crater, or the Great Caldron, we look down upon a succession of beds of basalt and of volcanic conglomerate reposing upon a single bed of trachyte, which latter would therefore seem to have been upheaved from the bottom of the ocean to the height at which we now observe it.

If, therefore, it be considered logical to extend to the older lava-beds of Vesuvius the same explanation which we adopt with reference to those found within the compass of our own observation, it would seem not less so to infer that the superincumbent beds of volcanic materials which, in the instances just quoted, rest upon the trachyte, have been tilted up by the same movement which upheaved the latter.

At Teneriffe, Sir Charles Lyell, although disputing Von Buch's elevation theory as applied to that volcano, admits that the whole island may have been raised bodily out of the sea by an upward movement. According to his views, indeed, this movement was a gradual one, a position which no one can gainsay in the case of a volcano elevated before the memory of man, but which seems untenable in the instances before cited, where an island like Sabrina made its appearance in a single night, or, like Graham's Island, in the course of a few days.

Mr Scrope has also omitted altogether to explain the formation of those crater-shaped cavities which occur in certain volcanic districts, as, for instance, in the Eyfel country, the elevated borders of which are composed exclusively of the rocks of the country, without any admixture of volcanic matter. Such is the fact with the circular volcanic lake called the Meerfeld; and the same remark, according to Mr Scrope's own description, applies to that called La Gour de Tazano,* in Auvergne.

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* Volcanoes, p. 48.

Indeed, many circular valleys in various parts of the world have been attributed by geologists of high authority to an upheaving of the surrounding rocks, such, for instance, as that in which the acidulated springs of Pyrmont are situated, and those to which Dr Buckland assigned the name of valleys of elevation in this country. Such events, of course, having taken place at periods of time long antecedent to historical records, cannot be educed as independent proof of sudden elevation ; but they at least show that, in interpreting volcanic phenomena, the analogy of nature does not limit us to that one mode of accounting for the formation of such mountains which is exemplified in Vesuvius or Etna at the present day.

Having now alluded to certain facts which have been altogether passed over by Mr Scrope, I will next notice others which appear to me difficultly reconcilable with his hypothesis.

That a body of semifluid materials should have been heaved up by the force of elastic vapours acting from below, so as to form a conical or dome-shaped mass elevated many thousand feet above the level of the contiguous country, and yet, owing to its vicinity, should have been confined to the area which it first occupied, is a supposition not unencumbered, indeed, with difficulties, but at least in no glaring opposition to mechanical or chemical laws ; and hence, in such cases as the five dome-shaped mountains in Auvergne, of which, and of the contiguous hills, Mr Scrope in his beautiful panoramic views has furnished us with so graphic a delineation, I should not scruple to adopt it as preferable to any other that has yet been proposed.

But that a body of lava, fluid enough to have been ejected from the interior of the earth as a lava current, should have gradually accumulated round a sort of central nucleus in such a manner as to build up by degrees a mountain of so great an elevation, and yet that it should never have diverged in any one direction, and produced a stream flowing either to the right or to the left, is to my mind as inconceivable as it is unprecedented.

The nearest approach, indeed, to a parallel case which Mr Scrope has been able to adduce is the pillar of lava forty feet high on the flank of Mauna Roa, which Mr Dana describes as produced by successive jets of viscid matter congealing one over the other. But the difference between 40 and nearly 5000 feet is so great, that we must demur as to extending to the latter the hypothesis which we apply to the former, especially considering that we should have to stretch it still farther in order to meet such cases as that of Chimborazo, the whole of which mountain, though no less than 21,100 feet in height, is stated by Humboldt and others to be composed of a species of trachyte, without any vestige of a crater or of ejected materials being found in connection with it.

It was with these gigantic phenomena fast in his recollection that Humboldt, the great and principal explorer of these extensive regions, conceived himself privileged to protest against theories founded only upon the observation of the volcanoes of Italy, and with a pardonable feeling of exultation at the wider field of induction which his own superior opportunities of foreign travel had afforded him, compared the geologist who imagined all the eruption rocks throughout the world to be moulded according to the model of those he was familiar with in Europe, to the shepherd in Virgil, who supposes, in the simplicity of his heart, his own little hamlet to contain within itself the image of imperial Rome.

At any rate, in the face of such facts as I have adduced, it would seem the most prudent course, in the present state of our knowledge, to keep in view the elevation theory as a reserve upon which to fall back, in case any of the phenomena of volcanoes should appear upon examination irreducible to any simpler and more familiar hypothesis.* The theory of upheaval, indeed, must not be considered merely on its own merits, but as constituting a part of a larger question, which cannot yet be regarded as disposed of.

If the strata of the globe generally have, from time to time, been effected by paroxysmal action, it can hardly be denied that volcanic rocks would be of all others the most likely to come in for their share in these great movements; and although the tendency of the elaborate researches and acute reasonings of Sir Charles Lyell and his school has been to show that the slow and gradual operation of subterranean forces may have brought about a vast number of those changes which affect the earth's surface, I am not aware that the great body of geologists are as yet prepared to admit that the same forces cannot have operated here and there in a more sudden and violent manner.

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* By reference to my work on Volcanoes, pages 622 *et seq.*, it will be seen that this was the point of view in which I contemplated the elevation theory long ago. Although there might be no direct proof that the older lava-beds, which form the bulk of volcanoes now in activity, had undergone upheavement, I should have at all times preferred supposing them built up in the same manner as the more modern ones, had not in some cases their rapid slope appeared to present a difficulty in the way of this explanation.

Indeed, the more enlarged views which men of science of the present day entertain with respect to the past duration of the globe—to the prevalence of which none have contributed more than Sir Charles Lyell and his immediate followers—prepare us to admit the probability of greater convulsions having taken place than any that we are actually cognisant of.

For what, after all, is the historical epoch within which our experience is necessarily circumscribed, but a mere speck in the series of past events, and therefore one by no means likely to represent to us all the possible phases through which the crust of the globe may have had to pass in arriving at its present condition?

The laws of nature, indeed, are uniform and constant; but does not our experience of the sudden effects of earthquakes prepare us to expect paroxysmal effects as a part of nature's economy; and do we not perceive as great a difference in the mode of action, when we compare the convulsive energy which, after the occurrence of a succession of earthquakes, elevated a line of coast on the shores of Chili in 1822–23, with the slow and tranquil upward movement which tends imperceptibly to raise the coast of Sweden above its former level, as between the gradual building up of a volcanic mountain by successive additions of lava currents, and scoria, and the elevation of a whole tract by a single burst of expansive energy?

The general question, however, as to the probability of paroxysmal action having occurred is one upon which the opinion of a mere geological amateur like myself will weigh but little; nor, indeed, even in that more limited field on which alone I may be considered as at home—namely, that of volcanoes—should I pretend to more than to caution geologists against the mistake of running from one extreme to the other, and after adopting to the full the particular views of M. de Beaumont with regard to the formation of craters, to hurry at once to the conclusion, that all the phenomena we observe may be explained by the mere pouring forth of streams of lava and ejections of scoria.

Granting that the volcanoes we now see in activity may all be referred to this mode of operation, where are the traces to be found of a similar series of phenomena in the older volcanic regions of Mont Dor and Coutal?

For although Mr Scrope has boldly assigned them all to those distinct volcanoes which he assumes to have existed,—namely, those of Mont Dor, Coutal, and Mont Meyer,—yet, as he has not undertaken the task of referring in detail the volcanic rocks of each district to its respective origin, as lava-beds proceeding from one or other of these supposed vents, I must for the present retain my scepticism as to their having been formed in the manner he imagines, and adhere in this instance to the hypothesis suggested by M. de Beaumont, that they were spread almost horizontally over the surface of the subjacent granite, and afterwards heaved up into the position in which we now find them by some force acting from beneath. And if we extend our view beyond the range of those igneous rocks which show a certain resemblance to the ones produced under existing circumstances, can we feel confident that, amongst rocks subjected to the incumbent weight of many miles of ocean, the same series of operations which we witness in our subaërial volcanoes would repeat themselves in an unmodified form?

Is it not rather more likely that, under so enormous a pressure, sheets of ignigenous materials should spread themselves over the bottom of the sea, until, by their accumulation, the repressive action became so great that it could only be overcome by the elevation of a volcanic mountain to the surface, and by the establishment of a permanent vent?

I offer these considerations as furnishing at least an apology for not withdrawing those passages in my work which have reference to the elevation theory.

In a descriptive treatise like my own, the true business of the author ought, I conceive, to be that of placing before his readers, not only all the well-established facts, but also all the tenable hypotheses which have been put forward to account for them; and although even the great names of a Humboldt or a Von Buch would furnish no excuse for clinging to an exploded error, yet the knowledge that such men as these adhered to the end of their days to a certain view with respect to the nature of volcanic operations, should render us more rigorous in requiring from those who reject it that all the phenomena should be distinctly referred to some other cause.

I have indeed to express my regret that, owing to absence from England at the time when Mr Scrope's paper was read before the Geological Society, I had no opportunity of making any remarks upon its substance in his presence; nor could I have done so subsequently before the same audience, because, as the admission of a purely controversial paper at a meeting of a London scientific society is itself rather unusual, I could not presume to trespass upon the time of the members by bringing forward on a subsequent occasion the arguments that might be alleged on the opposite side, especially as they had for the most part been already laid before the public in the work on Volcanoes, of which the last edition appeared in the year 1843.

As, however, some at least of these arguments do not appear to have been noticed in the Memoir referred to, I trust no apology is needed for submitting them to the Geological section of the British Association, where they will receive, I am sure, a fair and impartial consideration.

Vegetable Morphology—its General Principles.

By Dr Macvicar, Moffat, N.B.*

The forms of plants in general, the plant-form, why is it what we find it to be, and not otherwise?—that is a question which science has not yet answered. Philosophical botanists have indeed shown that all the more perfect plants may be regarded as consisting of an axis with its appendages; and that all these appendages, however varied in their forms and functions, are either leaves or transformations of leaves. They have also shown that all the special organs of plants have their uses, uses often manifold, and always good, and that the whole vegetable kingdom is beautiful, and calls upon every beholder that possesses sensibility to adore the Creator. But it has not yet been shown why the forms and organs of plants are what they are, and not otherwise; why the typical plant consists of an axis tending to spread out and radiate upwards and downwards into branches and root, the former tipped by the foliage and fruit, the latter by the rootlets and spongioles; why plants consist of the matter of which they do consist, and not other matter; and why they are so highly coloured and so fragrant. For all these features of the vegetable kingdom, and others of the same order, it has hitherto been possible to assign, not physical and physiological, but theological and moral reasons only. It has been possible to refer them only to the will of the Creator that they should be as we find them. Now, this is no doubt the ultimate reason; and for moral purposes, and for men in general, it ought to be sufficient. But to the man of science it is simply equivalent to saying, "God knows;" for the man of science is not at liberty to forget that whilst the Creator is the absolute Will, He is also the Supreme Reason, and as such has implanted in the soul of man the instinct of Philosophy, whose calling is to lay hold of Nature and wrestle with her for light as to the reasons of things, and whose word to Nature ever is, "I will not let thee go except thou bless me."

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* Read before the Botanical Society, July 12, 1860.

Doubtless there is a sufficient reason why the plant-form is as it is and not otherwise; and it is for the philosophical botanist to discover if he can what that reason is. To this inquiry there is in fact a moral and a theological, as well as a purely intellectual stimulus. Thus the forms of plants, at first sight at least, seem to exist in violation of all wisdom; they seem to be the very counterpart of those forms which pure intelligence, contemplating excellence of form as such, points to as the best—the very counterpart of those which geometry and dynamics sanction. Thus, though they be so useful and so beautiful, they are of all things the most fragile and fading; they are the sport of every blast. Other beautiful products of nature, gems, for instance, or pearls, may be set in gold, and stored up or worn by many wearers without being worn out. They preserve all their charms for many generations. But the most beautiful flower, the most fragrant nosegay, is faded before the evening be over. Now, why is this? Constant observation of the fact may indeed have so familiarised us with it that we may never think of inquiring, or even deem it needless or strange to ask. But there can be no doubt that if, in perfect ignorance on our part of the fate of flowers, a lily or a rose were presented to us, we could not in the first instance feel grateful enough to him who had given us such an exquisite production of highest art, yet, as soon as we saw how it was going with it, our gratitude would soon give place to a still stronger indignation, that he had merely mocked us with the possession of a thing so fading as to seem worse than the want of it. Now, why is it so? Why is the plant-form so fleeting? I answer, because it could not be more solid or more lasting than it is, if the vegetable kingdom is to take its place in nature, and to fulfil its mission there—that is, to intercede between and unite in harmony the fickle fleeting air and the fixed earth. Plants do not exist in disregard of the laws of a pure morphology—that abstract doctrine of form and structure which geometry and mechanics teach, and which the forms of the heavenly bodies, and of all stable structures exemplify. Plants realize those very forms which are most stable and cosmical up to the full measure that is compatible with their place and calling in nature, and the end they are appointed to serve.

But here it may perhaps be thought that all this is no more than an affectation of mystery, a raising of difficulties where none exist. That the forms of plants should be fragile, it may be justly said, so far from being a fault in their construction, is the very circumstance on which their usefulness depends; for, to the very extent that they are easily destructible, they are suitable as food for animals, a class of beings higher in the scale than plants, beings possessed of sensibility, beings such that a state of physical wellbeing in them is a state of enjoyment to them, and so teeming in multitude, and so worthy of existence, that for their sakes it may be said, in a high sense, that, next to the glory of the Creator, creation exists, yet beings such that they are all in want of food, which ultimately the vegetable kingdom alone can supply, and which it does supply well in the very degree that it is fragile and easily destroyed.

Now, against all this I have nothing to advance. I desire rather to appreciate it to the full. But I maintain that we have not reached all the reasons, or even the primary reason, why an object is as it is in all its details, and not otherwise, when we have discovered its economic use whether to ourselves or to other animated beings which are denizens of the world along with us. Such interpretations are good so far as they go; but the vastness of nature, and the multitude of its relations, demand a larger view. Thus, as to the point in hand, if we regard the vegetable kingdom as fashioned solely so as to form the best food for animals, we are thrown aback and silenced as soon as we are called upon to turn round and mark the abundance of uneatable and poisonous plants in nature. We are obliged to confess that our explanation is good only so far, but not adequate to account for the whole. The truth is, that we must keep constantly in mind that creation is a manifestation of other attributes of God as well as His goodness, and specially of His unity and immutability—in one word, His perfection. Hence in nature a pervading unity of structure, and an universal harmony or homology of form; and hence, on the part of the student of nature, the indispensable necessity of a doctrine of general homology, as well as of specific utility. With regard to the forms of the vegetable kingdom, for instance, besides their relation to animals as food, they exist in many other relations; and we may be sure that as plants existed before animals, there will be an antecedent, a more general, and a more purely intellectual, geometrical, or dynamical reason for them—a reason which will be as satisfactory to angels as to men, satisfactory to those who have no need of victuals, and who possibly may have but little sympathy with creatures who lay such stress upon victuals as we inert and gravitating mortals on the surface of this planet are obliged to do. Nor let the discovery of such more ample reasons be despaired of. The wonderful, the beautiful fact is, the number of ends, each great in its own sphere, which are obtained, nay, as it were, spontaneously fall out, through the fulfilment of a single law, when the position of that law is supreme. It is the same in the moral world; but that by the way.

What I desire now to affirm is, that in reference to the vegetable kingdom, as in reference to every realm in nature, there is a supreme law; and that in so far as it is purely determinative of form, it is purely morphological, purely mathematical and dynamical. The properties of form and structure, viewed in the light of pure intelligence contemplating a system, a unity, expanded or to be expanded in space and time, are never, either here or elsewhere, violated for the convenience of the individual. Universal order is never sacrificed to private advantage. Euclid of old, when he was inquiring into the first lines and properties of form, and composing his immortal work in the light of abstract intelligence, and so that it should culminate and close in the discussion of the five regular bodies, was paving the way, the only way, for the right understanding of nature. And alas! after more than two decads of centuries, we have now to take up the subject very much where Euclid left it.

The supreme law to which I now refer is this, that every individualised form in nature shall tend towards that which intelligence gives as the most perfect of forms, and shall attain to that form so far as is compatible with the nature and environments of the form-possessing object, as being also something else and something more than merely a form.

That such a law has every *à priori* argument in its favour, will not be denied. And let it not be denied or set aside at once by the fact, which may be alleged, that the forms of natural objects are just what the physical forces make them to be. I am prepared to show that the physical forces are themselves not only the subjects of this law, but the very instruments appointed to realise it in nature. It is only for its own misery that modern science, in the heads and hearts of so many of its cultivators, tends so often to rest in the physical forces as a last word. The physical forces are creations of pure intelligence and representatives of it, and they do nothing but in fulfilment of its behests and in execution of its designs. To rest in the physical forces, and to look to them as the first of things that we as men of science have to do with, is to consent to an ill-understood multiplicity of agents at the fountainhead, instead of a perfect unity. It is consequently to disregard the highest aspirations of the logical faculty, and to be intellectually miserable. Unless, as naturalists, we are free to trace creation up to that Unity to which it owes its being, and to seek at least, if not also to find, manifestations of the attributes of that Unity all down the stream of being, natural history renounces its title to a place among philosophical pursuits.

But what is that form which intelligence declares to be the most perfect as form, and in which I maintain that the first lines, the most general features of a truly scientific morphology are to be sought and found? To this I answer, that were it not that demonstration is needless, because it has been demonstrated so often before, it might be demonstrated here, that that form is the sphere.

But it is here to be remarked, that of spheres considered as realised in matter, two kinds are possible. There is *first* the solid sphere, or sphere commonly so called; and there is *secondly* the hollow sphere, or spherical superficies, or sphere properly so called. And of these two it is to be remarked, that while, viewed as composed of material particles (each particle a centre of attractive and of repulsive force), they both possess mechanical stability in a high degree, yet as to contents each is curiously the complement of the other. Thus the solid sphere is the form under which any given quantity of matter displaces least of the surrounding matter, and under which, consequently, any given quantity of matter can be stored up in the least bulk. It is therefore the fittest form for being chosen as a deposit of precious matter, when that matter is not in use. The hollow sphere, on the contrary, is the most capacious vessel into which a given quantity of matter can be fashioned without breach of continuity. It is the form by constructing which most can be made of the material employed. Its mechanical strength also, in relation to pressures and other disturbing forces, whether without or within, is a maximum. Between them both, these spheres fulfil, to a wonderful extent, the conditions of perfection of form.

Now I maintain that these two are the forms which it is the primary office of the physical forces to develop, so far as circumstances do not forbid their development. The proof of this I cannot enter upon here in detail; but this may be here remarked with regard to these forces, that however manifold their names, they are all of the nature of attraction or repulsion. Now attraction, as has been demonstrated since the days of Newton, and might have been inferred from the first morning that a dewdrop was observed, has for its first function to fashion all individualised portions of matter into solid spheres. Repulsion, again, has no less obviously for its first function to expand these solid into hollow spheres; so that between the two they constitute a complete apparatus for the development of these most perfect, most generalised forms, and for rendering them the forms of universal culmination. Nor is this all: if attraction and repulsion be not co-ordinate in extent and force,—if attraction be appointed to rule on the great scale and at first, and repulsion on the small scale and at last,—then these two forces not only give a contour to natural objects, they give also a course to nature. They prescribe as a rule, that an object shall be first constructed as a solid sphere; and that then, after being as such the representative of the prevalence of attraction, its particles shall tend to expand, and its form to develop, so as to distribute themselves in a spherical superficies, the object thus becoming the representative of the prevalence of repulsive power, or heat.

Now, in these facts and inferences an account of the first lines of vegetable form and life is to be found.

In keeping with what has been said of the solid sphere (that it is the fittest shape for a deposit or store of such matter as it consists of), it is seen to be the choice of Nature for the form of the vegetable being when deposited anew in the soil, when on its travels from one locality to another, when housed in winter quarters, and, generally, when the aim of Nature is to store up as much living matter as possible, so that it shall displace least of the surrounding matter and expose itself least to external injury. So far as the mode of nutrition and the type of the species permit, and as often as there is unity in the organ, the solid sphere is the culminating form of fruits, seeds, spores,* tubers, buds, &c. Nor is this all the verification which our theory derives from the phenomena. In accordance with its doctrine (that the course of subsequent action consists in the expansion of the material constituting the solid sphere into a hollow sphere, so far as the conditions of existence permit), the germination and evolution, the growth of the plant is but the protrusion and development of the contents of such solid spheres or spherules as have been named, with assimilation of surrounding matter. That the reproductive forms of plants are more dense than their other living forms generally, is matter of common observation. Vegetable matter in general floats, but seeds sink, and in fact their economic value is usually estimated by their density. They have invariably contents which they tend to protrude.

Moreover, the limit of form towards which growth tends is nothing else but the hollow sphere! In consequence of the extreme difficulty of constructing this form, it is indeed, when not of microscopic minuteness, usually reached only piecemeal, only in morsels, only by the unfolding of small disks (leaves) supported on radii (axes, branches, petioles), to which the peltate leaf or system of leaves terminating the branchlet or petiole is normal, as the spherical surface always is to its radii. Many, indeed, are the obstructions to the development of a spherical contour, many the impediments in the way: as, for instance, the structure of the embryo, and the specific development proper to it; the supply of food, not equally all around but in certain directions, and sometimes in one only; the embarrassment of the individual plant-form in its relations with other plants, with the ground in which it grows, the weather, &c. Still, with all these limitations, it is remarkable to what an extent the spherical is actually attained in the contour of fully developed outstanding plants and trees, as also the hemispherical in those which grow in tufts or clumps. The primary axis which carries up the first foliage into the air, does indeed often keep the lead which it takes at first, thus giving as the geometrical form which circumscribes the tree, not the exact sphere, but a spheroid (the form which is nearest to the sphere), the longer axis perpendicular to the horizon. Let but the eye, when wandering freely over Nature where her forms have not been modified by the artistic but mutilating hand of man, only mark the general contour of plant and tree, and construct in imagination the geometrical form by which the plant-form in the eye will be best circumscribed, you will wonder how often the circle in profile, the sphere in full form, is called for; and if not just these forms exactly, then those which constitute the least departures from them,—the ellipse, the spheroid or ovoid, the semi-ellipse or flattened tuft or clump or cone.

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* This would be the place for an allusion to the pollen also, were it not that this product of vegetable nature requires a separate consideration.

This, the first law of vegetable morphology, or rather of morphology in general, as illustrated by the vegetable kingdom, enables us also to explain in a satisfactory manner a phenomenon observed in simple plants or plants with a single axis, which in itself has been considered as strange, and seeming even to interfere with specific identity of form. Thus it is generally to be remarked of simple plants, and the fact is always introduced into drawings of ideal types of plants, such as those which are figured in the popular works of Schleiden and Unger, that their lower and upper leaves, whether viewed in reference to their disks or their petioles, are very imperfectly developed compared with those about the middle axis. The upper and lowest, in fact, are often quite simple, and want petioles altogether, though those in the middle between them be finely divided and fully petiolated. Now, what is this production of leaf-stalk and foliage about the middle of the stem but a normal development of radial and peripheral matter, bent on reaching and covering as far as possible the equatorial region of the plant-sphere periphery, a region which, being at once the largest and farthest from the axis, is most difficult to fill up and to reach? The phenomenon is usually explained by a reference to the condition of the vital action of the plant at different seasons,—its feebleness towards the beginning and the end of life, when the first and the last leaves are protruded. And no doubt the life of the plant is always co-ordinated with the work which it has to do. But why is life feeble at first, or why are the lower leaves developed when life is feeble, and why the same with regard to the last leaves, when the plant is touching on its full development? Why but because that life, that energy, has a certain design, a certain law to fulfil; whereof the hollow sphere is the most general and the most perfect expression among all possible forms. The very same thing is in fact observed among forest trees and perennial plants, to which this doctrine of feebleness at first and exhaustion at last does not apply.

But in what has preceded, I have taken for granted the existence of radii as well as a spherical superficies, of an axis as well as the foliage appended to it, of stem, branches, and petioles as well as leaves—of a scaffolding, in short, for supporting foliage widely extended in space, though belonging to a single individual, and though aiming at the formation of a single spherical shell of verdure. Now these radii, stems, branches, petioles, the law of sphericity can scarcely be held competent to supply. Were there no other law but that of the sphere which was determinative of the forms of vegetable nature, plants would be all parenchymatous and laminar, all leaf, frond, or thallus; the plant-form either successful in attaining the spherical form (as plant matter may, when individuality contents itself with minuteness), as cell or vesicle (*Sphæria*, *Sphærococcus*, *Hydrogastrum*, &c.), or unsuccessful, as is always the case where the plant is large, the *nisus* merely being indicated by the turning up or down of the edge of the frond, or the formation of a disk-like thallus, which is the first form of so many species,—now becoming a cylinder or tubular body (that is, a hollow sphere whose axis is indefinite), now a lamina turning round upwards and cup-like (*Cenomyce*, *Nidulariaceæ*), or pitted with lacunæ (*Sticta*), or turning downwards, or waved, or crisped at the edge, or over all the frond, as in many Algæ, Fungi, and Lichens. Now all this argues the influence of the sphere, and its power of direct self-construction without the aid of radii. And indeed to a much greater extent than in reference to the entire plant, the doctrine of the sphere accounts for the forms of the most fully developed and perfected parts of the thallophytes generally—those parts, namely, in which individuality has established itself most fully, and in which, consequently, the reproductive spherules or spores are produced. There is scarcely any of these tribes of plants whose forms do not culminate in spherical, hemispherical, or circular balls, shields, disks, or sporocarps of some such form, displaying lineaments of the sphere or its elements.

But it is equally certain that, from the simplest species up to the most perfect, the plant-form shows a disposition to ramify and to distribute itself as far and wide as possible in the medium in which it grows. In the very simplest organisms (*Confervaceæ*, *Hyphomyces*), ramification, radiation, is already carried very far. And although there has been a reluctance on the part of systematic botanists to recognise any analogy between this filamentation of these simple plant-forms and the branching of more perfect plants, yet, morphologically viewed, they are obviously and certainly analogous. Nay, among these simple plants, too, not only have we ramifications, but the rami or filaments even generally succeed in expanding at their tips either into laminæ exhibiting the forms of leaves (*Delesseria*) or into float-vesicles, which are hollow spheres or spheroids (*Sargassum*), or into multiple branchlets (*Polysiphonia*), or into spore-producing cells, as is general. On comparing the branching of a forest-tree between the eye and the horizon in a winter day, when the foliage does not intercept the sight, with that of a finely branched confervoid in water, in a glass vessel held up to the light, nothing can be more analogous than the two. They must be due to the same morphological law. Nay more, shocking as the assertion may at first sight appear, there is nothing for us but to affirm that the vital nodes in the stems of perfect plants, and the septa in the filaments of the simplest vegetations, are analogous, and do in point of fact owe their existence in both (as do also the analogous productions in veins, lymphatics, intestines, &c.) to the same morphological cause—and that the doctrine of the sphere, the tendency of every axis to become at once hollow and finite, so as to approximate the hollow sphere as nearly in form as it may, thus giving ends to itself and closing up in the line of the axis step by step as it lengthens, while as yet its length has exceeded as little as possible that of its diameter. But these things by the way at present. It is the very existence of an axis and branches, often long, tortuous, climbing, that we have now to explain; for this, which is nevertheless the characteristic feature of the vegetable kingdom, the law of sphericity does not explain. We might indeed affirm cogently in general, that the sphere gives its own radii, and therefore that the law which gives the foliage gives also the axis. But in actual nature the axis takes such a lead, ascends, spreads, creeps, at such a rate, that it is manifestly the illustration of some other law. Far from aiming at a minimum of space for the plant to grow in, the stem and branches seem to delight in extending and even often straggling farther and farther.

What, then, is the morphological law which gives axis and branches, diffusion and size to the plant, and which in fact modifies to such an extent the law of sphericity that, except in internal structure and microscopic species, it is realised only piecemeal, the foliage constituting a sort of dermo-skeleton or system of scales indefinite in number? This inquiry let us now proceed to answer. But here I would first remark, that this additional law, though it give the characteristics of the plant form, yet is not a law of vegetable morphology merely. It is like that which we have discussed—a cosmical law. But what is it? It has received many names, according to the point of view in which it has been regarded. Thus it has been designated now the law of continuity, now the law of diffusion, now of osmose, now of solution, now of crystallisation, now of chemical union, &c. I have elsewhere shown* that into them all the idea of assimilation enters, and that to include them all, and express the law in its most general and comprehensive terms, it must be called *the law of assimilation*. But as indicating a more purely mathematical conception of it, and therefore as more kindred with our former law, the law of the sphere, we may retain for it here the name of *the law of continuity*,—a name, moreover, which is consecrated by the invocation of the greatest philosophers of modern times, and especially Leibnitz. It is to the effect, that all abrupt and discontinuous movements and changes in nature shall be forbidden, and that dissimilars, on their mutual confines at least, shall be assimilated to each other more or less. It has been curiously verified in the laboratory in the phenomena of gaseous diffusion, liquid osmose, capillary action, &c. Nor has its operation been remarked on the small scale only. The relations of adjacent strata on the earth's crust supply many beautiful illustrations of it. But what we have here specially to remark is, that it takes place between the two great media that clothe our planet,—between the air and the earth, the incumbent atmosphere above and the soil in contact beneath. In virtue of this great law, the air on the confines of the earth tends to penetrate the earth, and to be assimilated to it by becoming concrete; while the earth, in its turn, on the confines of the air tends to penetrate—to ascend into the air, and to become aërial. Those earth-particles which are capable of the aëriform state tend to rise into the air as gas, vapour, odour; and those which are not volatile, yet separable from each other, tend to separate from each other, and to effloresce into the atmosphere, and to constitute, on the confines of the air, earth-tissues as highly diffused and lace-like, as spreading and elastic, mobile and coloured—in a word, as aërial and bright as possible. I say bright as well as aërial, because the atmosphere is the realm of light and colours as well as of air.

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* See Proceed. Roy. Soc. Edin., Sess. 1858–9, p. 146. Proceed. Phil. Soc. Glas. 1859, p. 52. Report Brit. Assoc. at Aberdeen, 1859; and *First Lines of Science Simplified*, &c., by the Author (Sutherland and Knox, Edin. 1860).

Such, under the law of continuity, the law of mutual assimilation, must be the tendency between themselves of these influential neighbours the air and the earth. The air-particles must seek downwards, and tend to become concrete like the soil; the earth-particles must seek upwards, and tend to become insulated as individuals like the air-particles, and to spread abroad in the air. Now, to what extent do we find this tendency actually realised in nature? To this it will be immediately answered, that the soil actually does absorb and retain in it a goodly quantity of air. The earth, also, it will be admitted, the longer it is exposed to the air, becomes more and more pulverulent; nay, actually rises in clouds of dust.

Certain earth-particles also, of peculiar tectonic powers (potass, lime, &c.), in secret places, where all is still, are known to effloresce beautifully into the atmosphere, suggesting as it were, and anticipating, the vegetable kingdom. Nay, what does not vaporise or effloresce invisibly, more or less? The atmosphere over damp clay has a clay odour, and an iron smell is affirmed to be perceptible over damp iron. It seems as if there were a tenuous invisible efflorescence around the most fixed bodies, not sensibly diminishing their weight however long investing them, which a continued evaporation could not fail to do, but merging, fusing, rooting them in the ambient air, and fulfilling as far as possible the law of continuity between them and the air. Moreover, the moisture of the terraqueous globe, under the same law, is ever rising into the air as vapour, and thus forms the world of clouds, so varied, so beautiful, so grand, and no less beneficent than beautiful. For having ascended into the air and gained the aëriform state, the vapour is now called upon by the earth beneath, and that under the same law of assimilation, to be assimilated to the earth beneath—that is, to become concrete, and to come down again. And, accordingly, down it comes, in rain, hail, snow, causing what to blind sensibility seems no better than a war of the elements, but to intelligence a harmony and a mutual embracing.

But this is not all. By the miracle of the creation of the vegetable kingdom at first, and by sowing the surface of the earth thereafter with the seeds of plants, the Creator has provided for the fulfilment of the law of continuity between the earth and the air to a wonderful extent. In fact, the vegetable kingdom as a whole, what is it, when viewed in reference to the atmosphere, but air become concrete as vegetable tissue, piercing down into the earth, and rooting itself in it? And what is it, when viewed in reference to the earth, but a system of earth-particles—aqueous, gemmeous, earthy, or alkaline—poised in the air, which, in so far as they are incapable themselves of the aëriform state, are suspended in and diffused through the air to the utmost by the foresaid scaffolding of concrete air-elements and vapours, and exhale into it in forms more truly aëriform than those in which they enter the plant:

fixed air exhaling as vital air, leaving its carbon behind; water becoming vapour; the vapour also resolving itself into oxygen and hydrogen, both of them more truly aëriform than the vapour itself; hydrogen, indeed, the most exquisite of all aëriforms, which, if it do not exhale from the plant into the atmosphere as pure hydrogen, it is only because it is obliged to take up carbon (one of the most fixed of all the elements) along with it, thus to render the vegetable kingdom fragrant, and to fill its cells with essences and oils, balsams and medicines manifold.

And thus we see that a plant must be animated by both an ascending and a descending system of parts and modes of action, of which the characteristics are, that the ascending system must be an analytic or separative, the descending a synthetic or combining agency. And thus we are fully able to understand how a living plant may accomplish such acts both of decomposition and of combination as cannot be at all imitated in the bottles of the laboratory.

But I confine myself here to remarking that, by this theory, by this additional law applied to vegetable nature, we obtain a full explanation of those features in the forms and structure of plants which the first law, the law of sphericity, fails to supply. That theory explains only the laminar, the parenchymatous, and the cellular. It does not explain the fibrous and vascular, or the existence of stem, branch, or root. But all these are fully accounted for by the law of continuity, the law of assimilation, if that law be allowed to take effect between the earth and the air. In a word, if a system of air-particles is to be made concrete in the earth and diffused there, and a system of earth-particles to be rendered as aërial as possible, and distributed through the air, what do we require as an instrument and a realization, but a radiating or branching root under ground, and a radiating or branching plant above ground, meeting in a stem which is common to both? While the Law of Sphericity gives the foliage and the general contour of the plant, the Law of Continuity or Assimilation (between the earth and the air) gives the stem, branches, and petioles, the root and rootlets.

To this theory there also attaches a standard, by which the place of a botanical species in the scale of vegetable forms may be determined. Thus, the more successfully a plant maintains a spherical contour, and at the same time ramifies, and subdivides, and multiplies its parts and organs, the more successfully it distributes its foliage and suspends earth-particles in the air, just so much more perfect is the plant as such, considered as an individual. And the same of the root, with regard to concrete air-particles.

Our theory also accounts for the substances of which plants consist, and the food which they require in order to growth, and therefore involves a theory of cultivation. All these points are at once indicated in our conception of a plant; namely, that it is an assimilative-diffusive apparatus placed on the confines of the air and the earth, and appointed to diffuse each in the other, and assimilate both. It is an apparatus (1.) for subliming earth-particles, for analysing and insulating them, for rendering them mobile like air-particles, and for carrying them up into the air; and (2.) for concreting air-particles, fixing them at the surface of the earth, and carrying them down into the earth as concrete matter. Hence we are able to understand why such large quantities of earths and alkalies should be found so constantly in plants, and are led along with the school of Liebig to affirm the necessity, in order to the successful cultivation of plants, of attending to the ashes of plants. But we are also prepared to find that, with the exception of the ashes, all the plant besides shall consist of concrete air-particles, or matter which the air can supply, and which can be best provided in the soil; for it is not to be forgotten that, with the single exception of atmospherical nitrogen, all the air-elements are earth-elements also, and belong to the earth as well as come into it by descent from the atmosphere. They are also the elements of which organic remains mainly consist. Our theory suggests, therefore, as the grand desideratum in agriculture, along with perfect tillage, not certain conditions as to the mineral constituents of soil merely, nor abundance of manure (considered as a combination of carbon, hydrogen, and oxygen, with nitrogen more or less) merely, but an adequate variety and abundance in the soil of *lakes*—that is, of air-elements in union with earth-elements—for the roots of plants to act upon, analyse, and absorb. And along with still improving methods of tillage, the construction of such lakes or dry manures with the greatest economy, and in variety answerable to the different crops which are grown, presents itself now as the great aim of agricultural science, nature having shown the example in giving guano.

But it is not the external forms and the chemical composition of the vegetable kingdom only which our theory explains. It throws great light upon the internal structure of plants. Thus it not only leads us to infer that all the first and simplest plants, and all the first elements in every plant, shall be little hollow spheres—that is, cells, vesicles, or utricles; but it leads us also to expect that, as soon as this cellular mass can claim individuality, and constitutes a plant at once aërial and terrene, with both a descending and an ascending system and therefore a combining or concreting, and an analysing or rarefying mode of action accompanying, the cellular matter, under the influence of the descending mode of action, commencing in the foliage, must, as it proceeds downwards, tend to concrete and combine into forms more and more continuous and dense, as, for instance, into vessels, fibres, and encrusting matter, still increasing in quantity as we approach the terrene part or root. Under the influence of the ascending, the separating, and rarefying system, on the other hand, the mass of cellular matter must continually tend to separate and expand into laminæ, or leaves and cells with their walls, still more and more bright and aërial (as in blossoms in particular, and parenchyma and the epidermis generally).

Under the same state of things, it follows that the distribution of woody (or concrete air) matter and of ashes (diffuse earth-elements) in plants and trees shall be the converse of each other. The woody matter, as the product of the foliage, and of the descending concreting system, will be found in greatest strength in the interior of the stem and root; the ashy matter or earth-particles, the product of the ascending system, in the periphery of the stem and of the entire plant or tree.

That all these deductions from our theory are verified by observation, is too well known to require to be stated; and here let us conclude with a remark suggested by the last inference, which throws light upon a great question in high philosophy.

It is well known, in accordance with what has just been shown, that plants and trees are aërial and light above, massy and strong beneath. Now, this fact in creation has usually, in common with others of the same order, been held to be fully explained by a reference to its expediency. It has been said that plants have been wisely made light and aërial above, solid and tough beneath, to the end that they may be able to support themselves and brave the storm. Now this is undoubtedly a good explanation so far as it goes; but from what has preceded, we find that it does not go to the root of the matter. From what has preceded, we find that the lightness of trees above and their solidity beneath is not a particular expedient adopted in their interest alone, for securing a special end in their behalf alone. We find that it is secured in the fulfilment of a grand principle—that it is provided for in an all-embracing law, in the framing of which this particular end and innumerable other beneficent ends were provided for. These ends may indeed be advantageously contemplated by us in detail as such. But if we are to look for such ends in every individual object in nature and in every organ, we are only preparing ourselves for frequent disappointment; for utility is not the point of view which ought *to rule* in our regards. In the natural as in the moral world, there is a higher principle than particular expediency or individual interest. There is a call all through nature, which is ever for order, universal order, the wellbeing of the whole. And accordingly there is in natural science a doctrine of *general homology* as well as of *special utility*. And truly wonderful it is to observe to what an extent, in the natural as in the moral world, multitudes of special uses and individual advantages in detail are secured as often as supreme law is obeyed. Hence the grand aim at once of science and philosophy ought to be, the discovery of supreme laws; and to this theme the preceding pages have been devoted in the delightful field of the vegetable kingdom.

*On the recent Earthquake Shocks in Cornwall, and Remarkable Whirlwind near Penzance.** By RICHARD EDMONDS, Esq.

The author, after referring to the earthquake shock in Cornwall on the 21st of October 1859, and the state of the weather on that occasion, as described by him in his paper read before the Royal Geological Society of Cornwall on the 28th of the same month, and printed in the "Edinburgh New Philosophical Journal," Vol. XII., pp. 1–14, thus proceeds:—

Remarkable as was the 21st of October 1859 for the suddenness with which cold weather set in, it was not more so than the 14th of December following, at 3 P.M. of which latter day a dreadful whirlwind, in the form of an inverted cone, white as snow, was seen near Penzance, travelling in a nearly straight line from north by east to south by west, at the rate of about 10 miles an hour, accompanied with a tremendous roar, from which the cattle in the fields fled with terror. The sound was compared to that of a dozen railway trains passing at one time over as many wooden viaducts, and was heard in several places 60 or 80 seconds before the whirlwind arrived. Its whiteness proceeded from the great quantities of snow it had caught up; much of which it afterwards formed into huge snowballs, and at intervals cast them to the ground. Its track from Trevayler to Zimmerman's Cot, a distance of more than two miles, was in many places strown with orchard trees and the tallest elms, which it had rooted up. The rookery at Trevayler, the seat of the Rev. William Veale, was the first place that suffered. Through the midst of it descends, in a straight line towards the north-east, an avenue or road bordered on each side with a row of tall elms. Down this road Mr Veale's coachman had gone that afternoon with his fowling-piece and dogs, but had scarcely reached the grove of ash-trees at the foot of it before he heard a most fearful roar, and soon afterwards, during a furious hailstorm, the trees of the grove were swayed to and fro in a most terrific manner, and the dogs gathered round him for protection. When the whirlwind had disengaged itself from the trees, he saw it rushing towards the south by west, carrying up the snow from the ground to a height of between one and two hundred feet. Its rotation was north, east, south and west, contrary to that of revolving storms in the northern hemisphere. On returning up the avenue, he beheld three of the elms on its north-western side torn up by the roots, and lying across the road with their heads towards the south, and an elm on its south-eastern side also torn up and prostrated in the same direction. When I visited the spot a few days since, I was struck with the remarkable manner in which the whirlwind had acted. It fell in an oblique direction first on the north-west side of the avenue, and I observed on that side as I walked down, first a tall elm half rooted up and almost ready to fall across the road. Five or six feet further down, on the same side, were the remains of another elm, which had been quite rooted up and thrown across the road. Then came four tall elms not at all injured, occupying 36 feet on the same side. Below these were the remains of two other tall elms, which had been rooted up and thrown across the road. Such is the description of about 80 feet of the north-western side of the avenue—its trees above and below this being uninjured. On the south-eastern side only one tree was blown down, and that was directly opposite the higher of the two last-mentioned uprooted elms. These facts seem to show that the centre of the whirlwind, where the four tall elms were left standing and uninjured, was comparatively powerless; that its great strength lay between 30 and 40 feet from its axis; and that its eastern side, by which the three last mentioned trees on opposite sides of the avenue were prostrated, was more powerful than its western side—this last circumstance being due probably to its rotating north, east, south and west, as it advanced southward, for the rotatory motion and the progressive motion would thus be combined on its eastern side to form its maximum power.

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* Read before the Royal Institution of Cornwall, at Truro, on the 11th of May 1860.

Similar effects were produced a mile and a half south of Trevayler, at the entrance-gate of Alverton House, at the west end of Penzance. A large elm about 40 feet eastward of that gate, and another tree about 30 feet westward of the gate, were blown down, but the intervening tall elms were left uninjured. In the same manner it passed through the orchards south of Alverton House, rooting up the apple-trees on its east and west, but leaving those in the middle of its path scarcely damaged. A woman was caught in it at Alverton; but, being near its centre, she escaped unhurt, although unable to breathe for a few seconds from the violence of the wind. Here, too, as at Trevayler, the eastern side of the whirlwind appeared more powerful than its western; and it is very remarkable, that at Trevayler and Alverton *all* the trees overturned, whether by the eastern or by the western side of the whirlwind, were prostrated with their heads towards the south, or between south and south-east. That they should have been prostrated in that direction by the eastern side of the whirlwind I can readily understand, considering that it rotated north, east, south, and west, and was advancing towards the south by west; but why those blown down by its western side should have been prostrated towards the south, or between south and south-east, is a question of less easy solution. I at first doubted the fact; but the occupiers of the orchard and the coachman at Trevayler were so clear and positive in their statement of it, that I have no reason for questioning their testimony. The whirlwind, after passing close above Higher Lariggan House, and destroying part of a field of broccoli plants as if a harrow had been drawn over them, proceeded down the hill to Zimmerman's Cot, in the orchards of which it overturned some fruit-trees, and snapped asunder the trunk of a large ash close by the road and stream running from Trereife Smelting-house to Newlyn. Beyond this spot it appears to have continued its course in the same direction to the sea, without doing much further damage. On the following day (the 15th of December), as stated in the newspapers, the shock of an earthquake was felt at Pately-bridge, and other places in Yorkshire. The snow, which set in at Penzance on the day of the whirlwind, continued there for a week—a very unusual length of time for this town. "The intense frost (says Mr Lowe of Highfield House), which set in with a rough N.N.W. wind on the 14th of December (the day of the whirlwind), reached a degree of cold on the 17th and 18th greater than ever recorded there in the month of December since 1841."

The earthquake shock through nearly all Cornwall, on the 13th January 1860, at 10.32 P.M. (local time), during a squally and unusually dark night, appears to have been the severest recorded in this county. It was felt at great depths under ground, in several mines very distant from each other, although not in any of the mines of St Just, on the west of Penzance. On the surface, however, in almost every locality, the persons who felt it were probably twenty times more numerous than those who experienced the shock of the 21st of October. I am not yet prepared with a full account of it, but hope to be so at the next meeting of the Royal Geological Society of Cornwall.

On the Structure and Development of Botrydium granulatum.* (Plate.) By GEORGE LAWSON, Ph.D., Professor of Chemistry and Natural History in the University of Queen's College, Kingston, Canada.

In prosecuting an examination of the freshwater Algæ of Lake Ontario, I have had a good deal of trouble in arriving at satisfactory results regarding a little plant growing on the lake shore, which I now believe to be identical with the "Bladder-headed Laver" found by Dillenius between "Newington et Hackney, prope Londinum." I presume, also, that it is identical with *Botrydium* (*Hydrogastrum*) *granulatum* of more modern botanists, although the conflicting descriptions and figures contained in works presently within my reach are by no means so satisfactory as the account given by the old cryptogamist of the last century. This, indeed, is the reason why I seek to place on record what I conceive to be a true explanation, so far as it goes, of the structure, mode of vegetation, and reproduction of this little plant, which seems to be as interesting to the botanist as *Amœba* is to the zoologist, a striking example of the manifold physiological phenomena that may be enacted by the very simplest apparatus of life.†

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* Read before the Botanical Society of Edinburgh July 12, 1860.

† Lindley observes—"One of the most remarkable plants of the order *Fucaceæ* is *Hydrogastrum*, which Endlicher describes as a perfect plant, with root, stem,

To the westward of Kingston, near Mr Morton's distillery, there is a flat piece of land jutting out into the lake, but protected from the action of the water by a barrier of shingle that has been thrown up by the waves. When an elevation of the water of the lake takes place (and this usually occurs, temporarily, several times a-year),* this bit of flat land is inundated or temporarily covered with water, like other low-lying portions of land along the lake shore. Its vegetation consists chiefly of a singular form of *Ranunculus sceleratus*, not more than three inches high, intermixed with *Veronica peregrina*, &c., and the pools and moist spots are covered with a profusion of Algæ, such as *Nostochineæ*, *Oscillatoriæ*, *Vaucheriæ*, *Desmideæ*, and *Diatomaceæ*. In clayey spots the surface is covered with patches or clusters of green glossy spheres, not much larger than pin-heads. This is the *Botrydium granulatum*, which is represented in fig. 1 in its natural site, the surface of a crust of mud. Fig. 2 shows the appearance of the plants as little *stalked* spheres, when seen in profile; and fig. 3 shows one taken apart, and the earth washed away from its minute radical fibres. The plants are rooted very firmly in the soil.

When viewed under a low power (as with a one-inch objective), the little plant is found to consist of an upper globular part, or head, with a more or less elongated neck or stalk, and a widely ramifying root, consisting of very delicate branched filaments, all as shown in fig. 4. Although these parts are distinctly enough defined, and have the semblance of separate organs, yet the whole plant consists of only one cell—there is but one internal cavity ramifying throughout the whole. This is filled throughout with a colourless, transparent fluid, slightly granular, as usual in cell contents. The head portion alone contains granular endochrome of a bright green colour, which, however, seems to be disposed as a lining on the inner surface of the cell wall, rather than to be mixed in-

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bud, and fruit, in imitation of the most highly developed races, but all produced by the branching of one single cell!" If we except the reference to a bud, the idea here expressed is not carried further than the real structure warrants.

* During the last year or two, a slight permanent rise in the level of the lake in this neighbourhood seems to have been going on, previous to which there was a subsidence.

discriminately throughout the cell contents, the bulk of which in the head (as elsewhere) consists of watery fluid. That the internal cavity of the plant is continuous, that there is no membrane or other obstacle separating the mass of green endochrome, may be readily seen by gently pressing the glass cover, whereupon the endochrome, previously confined to the globose part of the head, readily passes down the neck-tube, and finds its way into every ramification of the root, if the pressure be continued with sufficient force.

While the plant is immature, the endochrome does not present granules of any great size—the appearance, even under a one-eighth-inch objective of Grunow, being that shown in fig. 5. But as it gradually matures, it is found to contain spherical granules of larger size, which are filled up with green endochrome, often itself in the form of distinct chorophyll granules. It is these spherical granules, or gonidia, as they have been termed, that are concerned in the reproduction of the plant. They are represented in fig. 6. As the term *gonidium* involves theoretical considerations as to the genetic value of a body, I shall merely call them spherules.

From the above description, it will be seen that the mature *Botrydium* consists of a transparent sac, branched in the lower part, filled with fluid, and containing in the upper part or head endochrome, in which are numerous spherules. This sac, which is very tough and elastic, is distended with the fluid contents, and consequently presents a turgid appearance. Thus, if pricked with a sharp point, the sac bursts, and the watery contents are squirted out with force, scattering the spherules. This may probably take place spontaneously. When exposed to drought, the sac collapses, and allows exit to the spores by its gradual dissolution. But one of the most curious facts that I have to mention is one that probably explains the adaptation of the plant for its peculiar habitat. If a patch of *Botrydium in situ* is covered with water for a few hours, and then examined, it will be found that the sacs have burst spontaneously and scattered their contents, even although they did not appear to be quite mature. This result seems to depend upon a process of endosmosis. Moisture is absorbed through the whole surface of the plant, and to such an extent as to burst the already turgid sac, and thus the spherules are set free, and floated away from the parent, to form new colonies. While the collapsing of the plant by drought, and its gradual dissolution on the subsequent application of moisture, is one means of permitting the freedom and development of the spherules, the inundation of the plant's habitat by the water of the lake is a more speedy, and probably a more certain mode of determining the rupture, and transporting the spherules to suitable localities for germination.

These spherules, when carefully watched after their exit, are found to assume a new aspect. They gradually lose their spherical form, becoming more or less elliptical or elongated, and then passing through successive stages, indicated in figs. 7–14, until they have acquired the globose head, and neck, and root of the parent. The whole process of transition is so simple, that I need not do more than refer to the figures. If a process of impregnation takes place, I think it must be looked for *after* the spherules have quitted the parent sac. I have certainly seen phytozoid-like bodies *apparently* produced from the granular endochrome; but as to the contact of these with the spherules, and the effect thereof, this is precisely the point at which all such investigations become misty.

Several points remain still to be noticed.

Most algæ absorb nourishment through their tissues from the surrounding medium. This is not the case with *Botrydium*. It is furnished with an extensively ramifying root, the object of which is, not to spread over the surface, and give off buds for new individuals, as has been stated by some writers, but to enter the soil and absorb nourishment. Several authors have admitted this to a certain extent. Berkeley suggested the probability that "the rooting threads of *Botrydium*, *Caulerpa*, &c., do absorb nutriment from the soil, and perhaps for the reason that they are frequently exposed to the dry air, and would therefore wither without such a provision," &c. Not only is it capable of so absorbing nourishment; it is truly a terrestrial plant, furnished with a widely ramifying absorbing root, whose fibres do not contain endochrome; and it is incapable of being developed under water, for submersion has the effect of bursting its cell-wall.

Most authors regard *Botrydium* as unicellular, and truly so. Hassall, while merely quoting in the text brief characters from Greville and Harvey, gives a drawing (Plate 77, fig. 5) which by no means represents an unicellular plant, and I do not understand it.

While correctly describing this plant as developed from a "spore" or "gonidium," we find many authors also describing an additional mode of increase. This is best shown in Endlicher's figure (Lindl. Veg. K. fig. 9). In the words of Griffith and Henfrey, it is described as follows:—"The figure represents a specimen with a second budding from it by vegetative increase, and in this way the plants come to form tufts or groups like little bunches of grapes; hence the name" (Microgr. Dict. p. 103). In reference to this statement, I would mention that I have not been able to find a single instance of a bud arising or being given off in this way from a filament to form a new plant. It may, however, occur. But it must be observed, that the appearance of the plants in clusters does not depend upon such a mode of growth. If it did, we should have each cluster consisting of differently sized globules, according to their respective ages; whereas there is usually a general uniformity in size, showing that all the plants of each cluster are about the same age, and have probably arisen contemporaneously from one batch of spores.

I shall, in conclusion, offer a few observations on the nomenclature of the plant, which must be prefaced by a list of synonymes:—

Lichenoides fungiforme, capitulis vel vesiculis sphæricis aqueo humore repletis.—"Ray, Syn. iii. p. 70." (Dill.)

Tremella palustris, vesiculis sphæricis fungiformibus. The Bladder-headed Laver.—Dillenius, Historia Muscorum, p. 55, t. x. fig. 17.

Ulva sphærica aggregata.—"Linn. Fl. Suec." (Linn. Sp. Pl.)

Ulva granulata.—Linn. Sp. Plant. ed. 3, t. ii. p. 1633. Syst. Veg. Lichfield ed. vol. ii. p. 831. Œder, Enumer. Pl. Fl. Danicæ, p. 14. Lightf. Fl. Scot. 2 ed. vol. ii. p. 976.

Tremella granulata.—Linn. Syst. Nat. ed. Gmelin. Reg. Veg. tom. ii. p. 1446. Hudson, Fl. Anglica, p. 566. Wither. Arr. Br. Pl. 3 ed. vol. iv. p. 80. Roth, Sims' Ann. Bot. vol. i. p. 279 (description very good).

Ulva radicata.—" Retz. in Act. Holm. p. 251" (Agardh).

Vaucheria radicata.—Agardh, " Disp. p. 22." Species Algarum, vol. i. p. 465.

Vaucheria granulata.—" Lyngbye, Hydroph. p. 78." (Ag.)

Linkia granulata.—" Wiggers, Prim. Fl. Holsaticæ, p. 94," according to Agardh, but not of Micheli, nor Roth. Consult Sims' Bot. An. vol. i. p. 269, &c.

Botrydium argillaceum.—" Wallr. Ann. Bot. p. 153." (Ag.)

Hydrogastrum granulatum.—" Desv." " Endl." Lindl. Veg. K. 3 ed. p. 21, fig. 9.

Botrydium granulatum.—" Grev. Alg. Brit. p. 196, t. 19." " Hook. Br. Fl. p. 321." (Hass.) Hassall, Brit. F. W. Algæ, p. 305 (pl. lxxvii. fig. 5, is unlike the Canadian plant). Mohl, Veg. Cell. p. 3, fig. 1. Berkeley, Int. Crypt. Bot. p. 83, fig. 24. Griffith and Henfrey, Micrographic Dict. p. 103, fig. 75.

" *Gongoseira clavata*, Kutz. ?" (Hass.)

Although some modern works on Algæ do not contain any but recent references, it will be seen from the above list that this plant was familiar to our early English botanists, and it was correctly understood by them so far as their means of observation permitted. They seem also to have vied with each other in giving it new names, most of which have proved unfortunate. The old descriptive names of Ray and Dillenius are good. Linnæus first termed the plant *Ulva granulata* (1764), and subsequently in Gmelin's edition of the " Systema Naturæ," we find it removed to the genus *Tremella*, the specific name *granulata* being still retained. These two names were followed by many authors, both in continental Europe and in England; but Retzius had at an early period (1769) described it under the name of *Ulva radicata*, and this, as a *specific* name, was subsequently taken up by Agardh in preference to the prior one of Linnæus. Another specific name (*Botrydium argillaceum*, Wallr.) originated about 1815. From this statement it will appear that whatever generic appellation is chosen, the proper specific name is the Linnean one (*U.*) *granulata.*

In regard to the generic name there is more difficulty. Our modern ideas of classification require that the plant should not remain either in *Ulva* or *Tremella*, and there seems also to be good reason for separating it from *Vaucheria*. It must, in fact, form a genus by itself. Of the special generic names that have been proposed for it, that which has priority is undoubtedly *Linkia* or *Linckia;* but that genus of Algæ, orginally proposed by Micheli, does not seem to have been intended by him to include this plant, much less to be restricted to it. On the contrary, Roth describes four species of *Linckia;* one of which he *compares* to *Tremella granulata*, L., in regard to *form* and *size*, expressly stating that it is distinguished from that plant by important characters which are detailed. Moreover, we find (Lindl. Veg. K.), not only *Linkia*, Micheli, among the *Nostochineæ*, but *Lynckia*, Lyngb., among the *Oscillatoriæ*, besides a "*Rivularia Linckia*, Roth;" *Linkia*, Persoon, in *Gentianaceæ;* and *Linkia*, Cavanilles, in *Proteaceæ*. Herr Link might well exclaim, "Save me from my friends!" The result seems to be that all these generic names are practically sunk into synonymes. Whatever group may be chosen by botanists to commemorate Link, it is evident that it cannot be the bladder-headed laver of Dillenius. The next generic name that appears is *Botrydium*, Wallr., which is expressive enough, and has been adopted by most English writers; but it was originally associated by its author with the unnecessary specific name *argillaceum*. Greville retained the generic term, and restored the Linnean specific name, and I hope in future the example will be followed. *Hydrogastrum* is more recent, and should be dropped; so also of *Gongoseira*, Kutz., if it refers to our plant, which seems doubtful.

I ought to mention that I have not had an opportunity of referring to the works in which correct descriptions of *Botrydium* are most likely to be found, viz. those of Dr Greville and Kützing.

The conclusions that seem warranted by the above observations are these:—

1. *Botrydium granulatum* is an unicellular plant.
2. It is strictly terrestrial, and is incapable of being developed under water, like most algæ.
3. It is furnished with finely branched root fibres, which enable it to absorb nourishment from the soil, like other land plants.
4. Reproduction is effected by means of young spherical cells,

formed in the endochrome in the interior of the parent one, which are set free at maturity, by the bursting of the cell membrane of the parent.

5. Even where the plant is not mature an inundation of the habitat by water bursts the membrane, and thus effects the liberation of the spores.

6. If a process of impregnation occurs, it probably takes place after the spherules and endochrome have been ejected.

7. The plant does not increase by buds given off from the radical filaments (as stated by several writers), so far as the author has observed.

Explanation of Plate II.

1. Crust of mud, with numerous specimens of *Botrydium granulatum* on its surface. Natural size.
2. The same seen in profile *in situ*. Natural size.
3. A single plant detached from the soil. Natural size.
4. The same as seen under a low power (one inch objective).
5. Endochrome from the globose head of the immature plant, as seen under an eighth-inch objective with low eyepiece.
6. Spherical cellules, (" gonidia," " resting spores "), from endochrome of mature plant. (⅛th inch.)

7–14. Spores or spherules in successive stages of development, showing the principal steps of transition into a plant (⅛th inch.)

On the Colour of the Rhone. By JOHN DAVY, M.D., F.R.S., &c., &c.

This river, as it flows out of the Lake of Geneva and through that city, is, as is well known, remarkable for the beauty of its colour,—a colour, for the most part, almost of as pure a blue as the sky overhead when unclouded; indeed, on comparing the two on a fine day, the one, to my eye, chiefly differed from the other in being of somewhat less intensity.

To what is this colour owing? Is it merely the property of the water itself,—its true colour, as seen flowing in a deep or full stream? Or is it caused by the presence of some adventitious matter, such as iodine, as has been conjectured?

I am not aware of any facts in favour of the latter supposition; all that we know for certain respecting water appears to support the former, viz., that blue is the colour of water when pure, and seen in mass or accumulated volume,—a conclusion previously and long ago arrived at by the author of "Salmonia."* In proof of this, it need hardly be mentioned that blue is the colour of the ocean out of soundings; that it is the hue of glaciers, and also of all deep lakes the waters of which are of ordinary purity. Such observations as I made at Geneva, in a recent visit to that city, accord well with the above inference. I shall mention a few of them.

1. Not only is the water of the river there perfectly colourless and transparent when viewed in a small quantity, as seen in a water-jar for instance, but also the bed of the stream generally is of a light-gray hue, almost white. A portion of it, the latter, taken from a shallow part at a short distance from the bridge nearest the lake, was found, on examination, to be composed chiefly of carbonate of lime in minute granules, mixed with a very little silicious sand and a very minute quantity of vegetable matter. This bed, therefore, by any light which it might reflect, would not alter the colour of the water; a portion of the specimen which I examined, placed under blue glass, did not in the slightest degree impair the purity of its hue.

2. Though the quality of the river-bed is chiefly that just described, there are some exceptions. In some spots, the bottom is covered with aquatic plants; these, as well as I could judge, looking at them through some feet of water, are of a dark green; in other spots, the naked bed is formed of matter of a fawn colour, or of a brownish hue. Over both, the colour of the river is also more or less exceptional. Over the weeds it has a darker colour than ordinary, is of a less distinct blue, there appearing as if clouded. Over the other kind of bed, the change of colour is to that of a tender greenish hue, and this in gradation from the purer blue,—a change probably depending on a gradual shallowing of the water, and alteration of the colour of the bed.

3. Where the river is deepest, and where also its current is most disturbed, its water most agitated, and consequently

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* See "Salmonia," 4th edit., p. 373 *et seq.*, where the colour of water is discussed.

least affected by transmitted light from the bed, there the pure blue of the stream is most distinct.

The variations of hue which the river exhibits in a slight degree and very partially, are seen in a more marked manner and to a greater extent on the wide expanded surface of the lake, owing probably to alterations in its depth, and to the variable quality of its bottom reflecting light. However explained, the effects under a serene sky are very beautiful; on no other lake have I ever witnessed the same in such perfection, especially the finer shades of blue and green, of exquisite purity and delicacy—and this whether distinct, irregularly intermixed, or by insensible gradations mellowing one into the other. In times of storms, with an overhanging dark sky, we may be sure that the aspect of the lake, like that of the ocean, is altogether of another character. A lady well acquainted with it from residing on its shore, made use of a strong expression when referring to the change: she spoke of its appearance, when so overcast, as "almost terrible, it was so dark;" showing, it may be remarked, that at least some of the beauty of colour of water, under a serene sky, is owing to the light of the atmosphere reflected from its surface.

The Lake of Geneva, the largest of the Swiss lakes, affords a striking example of the purifying influence exercised by it on the water which it receives and discharges,—allowing, by rest, the particles of matter suspended in its affluents to subside, and this without in any material degree affecting the chemical composition of the water. The Rhone, descending from the Alps is, where it first enters the lake, turbid and nearly of the same hue as the Rhine before it reaches the Lake of Constance, and from the same cause—the minute fragments, chiefly of clay-slate, mixed with particles of limestone and of quartz, in a state of mechanical suspension. These subside on rest, the finer limestone particles probably in the deeper parts, the larger particles of clay-slate in the shallower and higher, giving rise to different qualities of bottom,—which, affecting the light reflected from them, thereby modify the hue of the water. As regards chemical composition, probably there is little difference between the affluents of the lake and its great effluent. Both contain carbonate of lime held in solution by means of carbonic acid. Some of this carbonate, it may be, is deposited to form the bed of the river where the Rhone flows out of the lake—this owing in part to increase of temperature, and in part to agitation. If so, this may account for the slight degree of milkiness which I fancy is perceptible in the blue hue where the river is most rapid and broken, and also for the fine granular state of the carbonate of lime (as seen under the microscope) in the sedimentary deposit constituting the bed of the river.

The subject—the colour of the Rhone—recalls a like colour, and in as great perfection, which I have seen in pools of water in Cornwall, in the parish of St Stephen; pools the beds of which are lined with white clay derived from the decomposition of granite. These pools are artificial reservoirs made expressly for collecting this clay, which is of so much value in the manufacture of porcelain. Simple rest suffices. Though shallow—not more, I believe, than two or three feet deep—yet the water in them, when it has become perfectly clear by the subsidence of the clay, is of a fine blue seen at a certain distance, offering a very striking appearance contrasted with the general surface of the ground, and more especially with the reservoirs,—such as are adjoining, from which the water has been drawn off—of almost snowy whiteness. Is not the colour of the water in this instance, under the peculiar condition of resting on a pure white bed, explicable on the postulate that blue is the proper colour of water, and confirmatory of it?

On the Distinctions of a Plant and an Animal, and on a Fourth Kingdom of Nature.[*] By JOHN HOGG, M.A., F.R.S., F.L.S., &c. (Plate III).

I have for many years past, whilst examining some of the simpler living Beings of the Creation, experienced the very great difficulty of defining the characters which should point out with accuracy whether certain organised beings of the lower or primary forms of life belong in reality to the *vegetable* or to the *animal* kingdom; and, indeed, three years ago, the following statement on this subject was published by me, after long entertaining the same views:—"Although, strictly speaking, in nature there may be no actual distinction between these two kingdoms, and that *life* in the lowest animal and that in the simplest plant may be the *same*, both beings having the same properties of existence, in their receiving nourishment, in their power of increasing in size, in their propagation, as well as in their being subject to the same penalty of life—namely, *death*,—still the naturalist must endeavour to draw a line of demarcation between these two great provinces, for the sake of the arrangement and classification of the infinitely numerous living beings or organisms existing in the world. And for this purpose, the clearest and most certain distinction between an animal and a vegetable seems to be the presence of a stomach or a stomachic-sac and of a muscular (and nervous) apparatus in the former, and the entire absence of them in the latter." (*Trans. of the Tyneside Naturalists' Field Club*, vol. iii. p. 166.)

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[*] This paper was read to the Section D—"Zoology and Botany, including Physiology"—of the British Association, at the Meeting held in Oxford on June 28, 1860.

I have been also long aware that several foreign naturalists had many years previously introduced the *animal-vegetable* theory. This, which appears to have received very few or no active supporters in our own country, comprised the ideas that some minute organisms were at the first period of their existence *animals;* and that, after enjoying for a short time a locomotive animal state, they became true *vegetables*, devoid of all further locomotion, but fixed permanently by their roots or bases. Such living beings, organised creatures, or organisms, were by them termed *Zoospores* or *Zoocarps—i.e.*, "animal-seeds," or "animal-fruits."

Moreover, in a still earlier memoir, published so long since as the year 1839, in the "Linnean Trans." (vol. xviii. p. 376), I wrote, "Notwithstanding the power of locomotion has generally been accounted as one of the strongest tests of animal life, and that which constitutes the most obvious difference between an animal and a plant, still this power is not alone confined to the beings included in the first great division of nature; for many observers have witnessed it in subjects which pertain to, and really are members of, the second division, or the vegetable kingdom."

Thus locomotion, although apparently spontaneous, has long ceased to indicate the distinction of animality, which was relied on by Linnæus as his fourth character, in these words:—"*Animalia spontè se moventia.*" So, in like manner, iodine and starch (amylum) have, if not failed to settle, at least greatly weakened, by their presence in certain organisms which are clearly animal, the determination of decided vegetability.

Again, the four component or chemical elements, hydrogen, carbon, nitrogen, and oxygen, have been considered in the endeavour to point out the same distinction, but without positive success; for it is well known that in animals nitrogen (azote) is copiously present in their tissues, whilst in some plants a small quantity is discoverable. The former, also, in respiring, consume oxygen and exhale carbonic acid gas, though the latter beings, in the same process, give out oxygen.

Our distinguished zoologist Professor Owen, on this subject, thus well observes:—"Chemical antagonism fails as a boundary line where we most require it—viz., as we approach the confines of the two kingdoms. Wöhler has shown (in "Wöhler and Liebig's Annalen," p. 206, 1843), that some of the free and locomotive *Polygastria* eliminate pure oxygen as the ultimate metamorphosis of their tissues; and on the other hand, Drs Schlossberger and Döpping have proved (in same Annalen, bd. lii. p. 119) that mushrooms and sponges exhale carbonic acid. The green-coloured matter called "chlorophyll," which is common in most plants, exists in the *Polygastria*, in the green *Planariæ*, and the fresh-water *polype*."—Owen's *Hunterian Lectures*, p. 4, Invertebrates, 2d edit. 1855.

But the latter part of this extract, I must observe, is evidently to be received with some qualification, for surely all fungi and all sponges cannot be said to exhale carbonic acid. Indeed, with regard to the fresh-water sponge (*Spongilla fluviatilis*), I have some time since shown that living specimens, under the direct influence of strong sunlight, give out numerous bubbles of gas, which was clearly found to be oxygen. (See "Annals," and "Mag. Nat. Hist." 2d series, vol. vii. p. 192, 1851.)

So also many of the Infusorians, which Dr Ehrenberg has more recently termed the many-stomached animalcules (*Polygastria*), are known to exhale oxygen after the manner of true vegetables.

And likewise, I had previously demonstrated that the white or pale-coloured specimens of the same species of *Spongilla*, when placed in a bright sunshine, received a beautiful green colour, as in plants—that green-coloured matter being "chlorophyll," or, as others have named it, "endochrome," or "chromule." The experiments are detailed at length in the "Magazine of Natural History," vol. iv. (New Series), p. 259, 1840.

Hence, then, most observers would have no hesitation in placing the fresh-water sponge within the supposed limits or boundary line of the vegetable kingdom.

Further, Professor Owen, in his recent and beautiful book on Palæontology, gives us the following definition of a *vegetable*. When the living "organism is rooted, has neither mouth nor stomach, exhales oxygen, and has tissues composed of cellulose, or of binary or ternary compounds, it is called a 'plant.'"

And I here add his definition of an *animal*, taken from the same publication. When the living "organism can move, when it receives the nutritive matter by a mouth, inhales oxygen and exhales carbonic acid, and develops tissues the proximate principles of which are quaternary compounds of carbon, hydrogen, oxygen, and nitrogen, it is called an 'animal.'"

These definitions are on the whole the best which I have yet seen, but I am doubtful as to what degree of reliance can be placed on these chemical elements.

The two principal characteristics, however, of an animal are undoubtedly the muscular and nervous systems, which do not exist in a plant, and which Professor Owen has not included in those definitions.

Linnæus, in his 12th edition of his "Systema Naturæ," which was the last that he lived to correct, and which was published in 1766, very nearly a century ago, arranged all natural bodies into *three kingdoms* of nature. These are defined by him in these words:—

> "*Lapides* corpora congesta, nec viva, nec sentientia;
> *Vegetabilia* corpora organisata et viva, non sentientia;
> *Animalia* corpora organisata et viva, et sentientia, sponteque se moventia."

Or, as Professor Owen has thus translated them:—"*Minerals* are unorganised; *Vegetables* are organised and live; *Animals* are organised, live, feel, and move spontaneously." But, in the last definition, the word "sentientia," I conceive, is not adequately rendered by the English word "feel" alone; for it means, with respect to animals, something *more;* in fact, that they are possessed of, or endued with, *sensation;* that is to say, they can discern, or perceive by the *senses*,—all, or more than that of "feeling."

As anatomical investigations, accompanied with that greatly improved instrument, the microscope, have rendered, since the time of the illustrious Swede, and especially within the last twenty years of the present century, our knowledge of zoology and botany, both recent and fossil, more extended and accurate, the *definitions* given by Linnæus must at this day be considered as insufficient and much too concise. I have accordingly attempted to enlarge them, in the following manner:—

Minerals are bodies, hard, aggregative, simple or component, having bulk, weight, and often regular form; but inorganic, inanimate, indestructible by death, insentient, and illocomotive.

Vegetables are beings, organic, living, nourishable, stomachless, generative, destructible by death, possessing some sensibility; sometimes motive, and sometimes locomotive in their young or seed state; but inanimate, insentient, immuscular, nerveless, and mostly fixed by their roots.

Animals are beings, organic, living, nourishable, having a stomach, generative, destructible by death, motive, animate, sentient, muscular, nervous, and mostly spontaneously locomotive, but sometimes fixed by their bases.

I am well aware, that in characterising animals as "having a stomach," it may be said that certain of the lower animals do not possess that organ; yet by that word I mean not only a stomach in its strict sense, but also a stomachic cavity, bag, sack, or pouch. And I have omitted, as too uncertain, the character of a "mouth," because the orifice or entrance into a bare cell, even in the lower plants, may be esteemed by some as such an organ.

Next, with regard to a *fourth kingdom* of nature, some foreign naturalists have maintained, for some time, another kingdom in addition to the *three* which are usually adopted, and of which the definitions have been already noticed.

In the opinion of some writers, *two* kingdoms of nature might be held sufficient—namely, (1) the *Inanimate* and (2) the *Animate;* or, (1) the *Inorganic* and (2) the *Organic;* yet, for an entire century at least, three kingdoms have been most generally received.

In lately perusing Professor Owen's excellent work on Palæontology, which was first given to the world last year in the new edition of the "Encyclopædia Britannica," but now (1860) published, with a few additions, in a separate and handsome volume, I found that he has introduced the "Kingdom Protozoa" in his "Systematic Index" as the first, and placed it before the "Kingdom Animalia," which he has constituted the second in order.

Previous to the descriptions of the different subjects of these two "Kingdoms," the learned author thus writes:—"The *two* divisions of organisms called 'Plants' and 'Animals' are specialised members of the great natural group of living things; and there are numerous beings, mostly of minute size, and retaining the form of nucleated cells, which manifest the common organic characters, but without the distinctive superadditions of true plants or animals. Such organisms are called "Protozoa," and include the sponges or *Amorphozoa*, the *Foraminifera* or *Rhizopods*, the *Polycystineæ*, the *Diatomaceæ, Desmideæ, Gregarinæ*, and most of the so-called *Polygastria* of Ehrenberg, or *infusorial* animalcules of older authors."

And this is the arrangement which the same author has made for these organisms;—

"KINGDOM, PROTOZOA.
Class, AMORPHOZOA.
RHIZOPODA.
SUB-CLASS, POLYCYSTINEÆ.
Class, INFUSORIA."

But since naturalists are divided in opinion—and probably some will ever continue so—whether many of these organisms, or living beings, are animals or plants, the word Protozoa, *i.e.* first or "early animals," which was formed by a foreign naturalist, can alone include those that are admitted by all to be animals or "zoa,"* which are already members of, and included in, the Kingdom *Animalia*, and not those concerning which it is doubtful whether they be not rather "plants" or *phyta*. So, again, the word *Amorphozoa*, meaning "amorphous" or formless animals, which Professor Owen makes his first "class" of the Kingdom "Protozoa" is incorrect, for most of the organisms inserted in it are the sea and fresh-water sponges, the animality of which still remains in doubt; for many, including some of the most distinguished of foreign naturalists, as Agassiz, Von Siebold, Stannius, Van der Hoeven, &c., maintain their vegetability.

Also, the late Professor Ehrenberg accounted the *Desmideæ* and *Diatomaceæ* to be true animals, whereas several authors now assign to them a place among vegetables; and these more accurate observations confirm what I had twenty-two years before supposed would be the case; for I at that time said that some of those bodies then referred to the *Infusoria* would probably turn out to belong to *plants*.

Hence, it is evident that naturalists cannot agree in the adoption of such characters as are considered most satisfactory in limiting, or in drawing a line of demarcation between, these two kingdoms.

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* This is doubtless the proper meaning generally assigned to this word. Ζῶον, the substantive, is an "animal," but Ζωὸς, the adjective, which is rarely used, signifies "living;" see Homer, *Il.* ii. 699. Therefore, in this latter sense *Protozoa* would be the *first living* ("things" or "beings" understood). But this would certainly cause errors. So *Amorphozoa* would in this last sense mean "formless living" (things), including many lower *plants*; still this is not what M de. Blainville intended when he formed that word, because he expressly defines it to signify "*animaux* sans forme determinée."—*Vide* "Actinologie," p. 527.

If, then, as Professor Owen states, "as the knowledge of the nature of animals and plants has advanced, the difficulty of defining them has increased, and seems now insuperable" (Hunterian Lectures, *Invertebrates*, p. 2), it appears to many desirable to place those creatures, or organic beings, whose nature is so doubtful in a fourth kingdom. And although I at present do not feel quite convinced of the immediate necessity of doing so, or that it will ever remain—notwithstanding the progress which we hope will continue to be made in physical science—impossible for man to determine whether a certain minute organism be an animal or a plant, I here suggest a *fourth* or an additional kingdom, under the title of the *Primigenal* kingdom,

REGNUM PRIMIGENUM,
continens PROTOCTISTA, *i.e.*,
PROTOPHYTA ET PROTOZOA.—

This *Primigenal* kingdom would comprise all the lower creatures, or the primary organic beings,—'Protoctista,'—from πρῶτος, *first*, and κτιστὰ, *created beings*;—both *Protophyta*, or those considered now by many as, lower or primary beings having more the nature of plants; and *Protozoa*, or such as are esteemed as lower or primary beings, having rather the nature of animals. And to those formless or amorphous beings, whether partaking more of a vegetable or of an animal nature, I give the name of *Amorphoctista*—ἀμόρφοκτιστὰ—instead of *Amorphozoa*, originally bestowed on them by the French writer M. de Blainville.

But I must here mention that the word "Amorphoctista" only applies to the living beings of Sponges in their fresh state, and does not mean that the Spongiaries (*Spongiaria*), or skeletons, or remains of the sponge after the death and decomposition of the live jelly, or living being,—*Spongioctiston*,—are without form, for a great many spongiaries are not amorphous, but have very distinct forms.

The *Primigenal* kingdom might be placed either the fourth and last, or between the vegetable and the animal kingdoms.

So I may add the description of ancient or fossil remains, or created beings, whether the word "Palæontology" or its synonyme *Palæoctistology* be used, will of course include both *Palæozoology*, the account of ancient or former animals, and *Palæophytology*, the description of ancient or former plants; although I fear the word *Palæoprotoctistology*, signifying the account of former or ancient lower creatures, or primary organisms, would perhaps approach too near to the objectionable character of "sesquipedalia verba" to be brought into general use.

Since, indeed, the vegetable and animal kingdoms have been well compared to two lofty pyramids,* which diverge from each other as they ascend, but are placed on, or united in, a common base; this base, then, might fairly represent the *Primigenal* kingdom, which includes the lower creatures or organisms of both the former, but which are of a doubtful nature, and can in some instances only be considered as having become blended or mingled together.

The annexed diagram (Plate III.) will perhaps show more clearly the divergency and union of the several kingdoms, by the different colours that are used.

The *brown* represents the earth and the mineral kingdom; and which is drawn higher than the two pyramids, for the purpose of representing the highest peak or mountain on the surface of our globe, and as being *above* the known limits of either vegetable or animal existence. The *yellow* colour denotes the vegetable kingdom; the upper portion of the first pyramid is tinged gradually darker, in order to exhibit the highest or more perfect state of plants. The *blue* indicates the animal kingdom; the upper or dark blue signifies the more perfect condition of animality; whilst in both pyramids, as the beings descend towards their base, they lose by degrees their chief characters respectively; and this is designated by the paler yellow and paler blue; and at length these two colours gradually blend or unite, and so constitute together the colour *green* in the base, common to both pyramids.

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* I have made both pyramids of about an equal height, on the supposition that there is no great difference in the limit of life for plants and for animals, upon the most lofty mountains of the world.

This base is intended to represent the *fourth* or the additional kingdom—the 'Primigenal,'—and which contains those organisms to which I have assigned the name of *Protoctista* —whether such primary created beings be in part *vegetable* (Protophyta) or in part *animal* (Protozoa), but whose respective characters cannot be sufficiently determined, so as to place them higher in the scale of Nature.

*Notes on the Geology of Captain Palliser's Expedition in British North America.** (With Diagramatic Section, Plate IV.) By Dr HECTOR.

The following remarks are explanatory of a section commencing at Lake Winnipeg, continued along the basin of the Saskatchewan River to the Rocky Mountains, and from thence to Vancouver's Island. This section is only intended to represent the more general results of this geological exploration, as a preliminary to the reports which are in preparation.

The rocks east of Lake Winnipeg have been fully described by geologists. They are a part of the so-called Laurentine chain, and consist of granite and metamorphic rocks (*a*). On these lie Silurian limestones, cherty, and of magnesian character, with corals and shells, easily referrible to Silurian types (*d*). Above these Mr Hind has found Devonian strata, of which, however, I saw no trace farther south. The supposed line of their outcrop is marked by salt springs.

The first well-defined strata in the Prairie country occur 150 miles west of Red River, and are indurated olive shales, with ferruginous bands, and traversed by veins of clay ironstone, with a few small fossils, chiefly fish-scales, and a small, neat species of nucula. They are a deep-water deposit (*g*).

At the elbow of the Saskatchewan River, the banks are formed of purple laminated clays, with lines of Septaria of various sizes. These Septaria yield fossils, which are truly cretaceous forms. The most common are *Baculites* and *Inocerami*. These Septaria clays are also deep-sea deposits (*g*). They are again met with on the north branch of the Saskatchewan, 150 miles to north-west, and the course of this river is for some distance determined by these soft beds. At the Snake Portage, in lat. 54° N., I thought I observed them overlaid by thick grits and clays, which must be next described; but of this junction I am not certain, and the dip is so slight that they may be even underlaid by these grits.

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* Read at British Association on July 3, Oxford Meeting.

The latter strata (*h*), in beds often 200 feet thick, form high ridges, which range north and south, crossing both Saskatchewans, and also the Red Deer River, at the Nick Hills. They form mainly two parallel ranges, and between them occur clays with coal or lignite beds from 2 to 10 feet thick, and consistent in their strike from north-west to south-east. This coal is used at Fort Edmonton, and burns pretty well. Some vegetable impressions, like those of cypress and dicotyledonous leaves, are found in the shale, but no other fossils.

As these coal-beds and shales occur in the river-beds, and at low levels compared with the surrounding prairie, it is manifest that the surface-beds of which these are composed are of later age; but whether conformable with them or not, I am unable to say.

To the south-east of the elbow of the Saskatchewan, at the base of the Coteau de Prairies, and at a locality on the Souris River known as the *Roche Percée*, is a group of marls, with limestone bands, containing so much iron as to weather of a bright vermilion colour, and ash-coloured arenaceous clays, with their bands of lignite, and silicified wood (*k*). Selenite crystals are abundant in these marls, often clustered in stellate forms. They are mixed with bands of grit, from a few feet to 30 feet in thickness; and these being generally of a soft nature, with indurated portions, weather out in the most grotesque forms.

On the higher grounds traversed by Battle River, and again on Red Deer River, where they are seen to rest on the great lignite group, are also beds of marl, limestones with iron like those of the Roche Percée, beds of lignite and true brown coal, with silicified trees, and abundance of fossils of an estuarine character. Among these latter are oysters, a good deal like the Pacific species, *mytili*, *cyprina*, and other marine forms in some beds; in others, *paludina* is the prevalent fossil. On the very high grounds (such as the Ochèschis or Hand

Hills and the Cypreés Hills), these strata pass up into sands (*m*), gravel, and beds of coarse shingle, which, at the same level (4000 feet above the sea), skirt the base of the Rocky Mountains, and there rest on the edges of upturned strata of various ages.

All the strata which I have mentioned are covered with a mantle of drift, which does not rise much above 3000 feet; but near Battle River there seems to be a group of deposits which I have termed Tertiaries of the low grounds (*n*).

The strata composing the Rocky Mountains may be briefly described as follows:—In crossing from the east, thirty or forty miles before entering the range, beds of grits and shales are observed much disturbed, but obviously dipping to the east (*f*). From a level of 4000 feet above the sea, the mountains rise as parallel ranges of cliffs from 3000 to 4000 feet in height. The first five or six of these ranges are composed of blue crystalline and earthy limestone in bold plications, including portions of the same grits and clays that are seen along the eastern base. This group of strata must be several thousand feet in thickness, and contain fossils of Carboniferous age (*e*). To the west, and forming the range which in general determines the water-shed, is an immense thickness of quartzite and conglomerates, not much altered, and apparently horizontal (*d*). A wide longitudinal valley marks the line between this formation and the last mentioned, and is probably the site of a great fault.

On descending the western slope of the mountains, while in the bottom of the valleys are vertical talcose slates (*c*), the higher parts of the mountains are composed of the same strata which form the eastern ranges, until the great valley is reached, which the Columbia and Kootanie rivers traverse, while their course is parallel to the range.

West of this a belt of slates and semi-metamorphic rocks was crossed, followed by granite with true metamorphic rocks (*a*), containing serpentine and marble, which brings us to Colville.

South and west of this plain commence the great superficial floes of basalt with beds of tufa, which have emanated from the flanks of the Cascade range (*b*). The Cascade range itself consists of sienite and slates, with volcanic rock of recent date.

The greater mass of Vancouver's Island is composed of the same metamorphic strata as at Colville; but along both sides of the Gulf of Georgia, which separate it from the mainland, and also forming the islands in that gulf, occur beds of grits and coarse conglomerate, much disturbed and resting on volcanic rocks, and containing the well-known deposits of coal and lignite as at Nanaimo and Bellingham Bay (*h*). These coal-bearing grits at Nanaimo, I found to be overlaid by Septaria clays (*g*), such as those I have found to the eastward of the Rocky Mountains, and containing the same cretaceous fossils, comprising baculites and inocerami. These clays are observed, again, to be covered by grits. Fossils were obtained at some distance below the coal at the base of the whole group, which have not yet arrived in England for examination. They are, however, either lower cretaceous or oolitic forms.

Observations on some Bisexual Cones occurring in the Spruce Fir, (Abies excelsa).* By ALEXANDER DICKSON, Esq., M.D., Edin.

When in Peeblesshire, in the beginning of last month, I met with an interesting, although apparently not a very uncommon abnormality in the shape of what may be termed bisexual cones, occurring in some young spruce firs. The abnormality consisted in the lower portion of the cone being covered with stamens, while the upper or terminal portion produced bracts and scales like an ordinary female cone.

The staminiferous portion varied in extent from about ½ to ¾, or even more, of the whole cone, and differed in no respect from the normal male cone, except perhaps that it was rather thicker, in consequence of the axis being a little stouter than usual.

The remaining upper part of the cone, on the other hand, bore small, narrow, more or less acute, bracts, with large pinkish or rose-coloured scales in their axils, and in fact resembled the normal female cone.

On closer examination, I found the stamens at the upper limits of the male portion of the cone somewhat altered in shape. The indurated scale-like crest of the anther became more elongated, whilst the anther cells at its base were diminished in size. The stamen now closely resembled one of the bracts of the female cone; indeed, some of these taper-crested stamens contained the lowermost scales of the female portion in their axils.

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* Read before the Botanical Society, 12th July 1860.

In these specimens, therefore, *the stamens in the lower part of the cone are serially continuous with the bracts of the terminal portion.*

Schleiden mentions, that "in *Abies alba* it not unfrequently happens that a portion of the lower leaves of the female inflorescence become converted directly into stamens; but then no axillary buds [scales] are developed." * The abnormality to which Schleiden refers must have been precisely similar to that which I have exhibited; and I cannot but think, that if he had looked at them closely enough, he would have found some of the lowermost scales in the axils of stamens as I have done.

Richard, in his *Mémoires sur les Conifères*, &c., plate xiv., has given a somewhat indifferent representation of an "*amentum androgynum*" in *Abies*, of which the lower portion is staminiferous; but without any commentary, beyond the mere indication of stamens in one portion and scales in another.

Dr Lindley, in his "Vegetable Kingdom," thus refers to the morphological constitution of the male cone. "It is obvious, that in the larch, the cedar of Lebanon, the spruce, and the like, each anther is formed of a partially converted scale, analogous to the indurated carpellary scale of the females; and therefore each amentum consists of a number of monandrous naked male flowers, collected about a common axis" (p. 227).

That the above is an erroneous view of the homologies of the male and female flowers in the coniferæ, I am fully persuaded, and I am the more impressed with the necessity of attempting its refutation, when I reflect on the consideration which is justly accorded to Dr Lindley's opinions.

1. There is no reason why the stamens in a male cone should not be regarded as foliar structures belonging to one and the same axis, viz., that of the cone, since they are not

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* Schleiden's Principles of Botany (Lankester's translation), Note to p. 299.

placed in leaf axils, nor do they, so far as I know, present any articulation, or any other evidence of their being possibly other than what they seem,—viz., simple stamens arranged spirally upon one common axis, thus constituting a single male flower.

That such a view of the male cone is well founded, appears from the abnormality under consideration, which proves the stamens of the male cone to correspond to the "bracts" of the female. That the bracts of a female cone are the leaves proper to its main axis, is at once proved by examination of the larch cone, where there is a gradual and beautiful transition from the "bracts" to the ordinary green leaves surrounding the base of the cone, with which leaves the bracts are serially continuous. From these considerations it follows that the stamens in a male cone represent the leaves proper to its main axis, that, in fact, they collectively constitute a single male flower as I before mentioned.

2. In these bisexual cones, the "bracts" at the upper part are serially continuous with the stamens below. From this it is evident that the stamens in a male cone must be represented, morphologically at least, by the bracts in a female, and not by the scales, as Dr Lindley believes.

The question, however, remains—What is the nature of these scales?

As the bracts of the female cone are the leaves of its main axis, it is manifest that the scales which originate in their axils must belong to secondary or lateral axes, whether they are viewed as foliar or as axial structures.

The opinion generally entertained by botanists, that the scale is a foliar structure, analogous to a carpellary leaf, has been ably combated by Schleiden, who asserts that "throughout the whole vegetable kingdom, *no* simple leaf is *ever* formed in the axil of another leaf."* From this, among other reasons, he draws the conclusion that the scale cannot be considered as a leaf, but must be regarded as an axial organ.

Dr Lindley has attempted to negative the objection by asking "what the fruit of *Salix* is but *folium in axillâ folii?*" This question, however, regarding the fruit of *Salix*, seems irrelevant, since there is not merely a simple leaf contained in the axil of each bract in the amentum of *Salix*, but a floral axis with receptacular scales, carpels (probably two), and a placenta with ovules.

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* Schleiden's Principles of Botany (Lankester's translation), p. 385.

At the same time, Dr Lindley does not seem to consider the content of the bract in the cone as consisting *absolutely* of a simple leaf, since he says that the scales "occupy the same position with respect to the bracts as the leaves [I presume of *Pinus*] do to their membranous sheaths." The fascicled leaves, however, which occur in the axils of the membranous leaves in *Pinus* are, as he himself holds, the product of secondary or lateral axes; and the occurrence, indeed, of these short and more or less abortive shoots in the axils of membranous or bract-like leaves in *Pinus* affords a good argument in favour of Schleiden's determination of the scales of the cone as shoots, abortive so far as longitudinal extension is concerned, and comparable to the flattened leaf-like shoots of *Ruscus* and *Phyllanthus*, or to the curiously expanded shoots in *Phyllocladus*,* which, as being a gymnosperm, possesses a peculiar interest in this respect.

On the whole, as there is no evidence that the bract in the cone contains other than a simple structure—viz. the scale—and as we cannot consider this latter as a *simple leaf* ("*folium in axillâ folii*" being, strictly speaking, without a parallel), we are obliged for the present to accept, as more probable, the other alternative, and view the scale as a *simple flattened shoot*.†

I do not intend at present to enter at length upon the homological relations of the female flowers in gymnosperms to

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* With these last must not be confounded the somewhat similar expansions in *Salisburia*, which, however, are true leaves, producing shoots in their axils.

† I must not omit to mention, that Dr Lindley (Vegetable Kingdom, p. 227) refers to a figure in plate xii. of Richard's *Mémoires sur les Conifères, &c.*, where he says that there is represented a monstrous cone of *Abies*, in which the scales have assumed the common appearance of leaves. I am rather at a loss to account for Dr Lindley's reference, because the abnormality figured in plate xii. is not a cone, but what Richard terms "*monstruosité strobiliforme*," and consists in a hypertrophy at the bases of the leaves, resulting from irritation or morbid stimulus induced by the attacks of an insect.

In plate xiii. of Richard's work, there is a figure of a larch-cone, to which, more probably, Dr Lindley may have intended to refer; but in this, as in all those in the other Phanerogamia. The remarkable similarity which various observers have shown to exist between the *corpuscula* in the coniferous "ovule," and the *archegonia* upon the *prothallus* in the large spore of Lycopods and Rhizocarps, would seem to indicate analogies very different from those which have been sought for in the Phanerogamic sub-kingdom, and would even lead us to doubt if the so-called "ovule" in Gymnosperms be really equivalent to the ovule in the other Phanerogamia. In reference to this point, it would be of great importance to determine whether the ovular envelopes of Gymnosperms (in those cases where there is more than one coat) follow the same order of development as those in the other Phanerogamia. The discovery by Griffith, that the third and most internal envelope of the "ovule" in *Gnetum* is developed *after* the appearance of the two outer coats, seems to point towards an important distinction between these and the ordinary Phanerogamic ovules, where, as is well known, the coats are developed in order from within outwards.

There remains much to be done in elucidation of the morphology of the bractless, or, more probably, *scaleless* cones of the *Cupressineæ.* It is very difficult, indeed, to see how they are to be compared with the cones of our ordinary conifers.

In conclusion, I would state, that although as a general rule it is improper to insist strongly upon any monstrosity as proving a general morphological principle, yet, in the present instance, the abnormality only confirms what may be deduced from a comparison of the normal male and female blossoms in the coniferæ to which I have alluded above.*

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ordinary instances where a portion of the axis of a cone becomes leafy, the leaves continue the series of the bracts, whilst the scales are suppressed at that part; and, in reference to this case, Richard makes the following observations (p. 68 of *Mémoires*) :—

"1. Coni axis interdum summitate abit in ramulum foliosum; foliis solitariis distantibus.

2. Squamularum posticarum plurimæ in folii principium desinunt; supremæ etiam nonnullæ in verum folium conversæ. Ergo, non squamæ fructiferæ, sed posticæ tantum in folia mutantur."

This is quite conclusive—"Therefore, *not* the scales (*squamæ fructiferæ*), but only the bracts (*squamæ posticæ*), are converted into leaves."

* Since reading this paper, I have (in July) again visited the locality where

Some Points in support of our belief in the Permanence of Species, and on the very limited application of the doctrine of their Origin by Natural Selection, suggested by a discussion in Section D of the British Association for the Advancement of Science.

By LIONEL S. BEALE, M.B., F.R.S., Fellow of the Royal College of Physicians; Professor of Physiology and of General and Morbid Anatomy in King's College, London; Physician to King's College Hospital.

In the present state of knowledge, it is perhaps impossible to prove that all the different organic forms on this globe have not arisen from a process of natural selection; that we have not all descended from one or more principal primary forms; that our origin, development, and death are not dependent upon external conditions alone; that after the lapse of a vast epoch, man shall not himself give place to a creature upon this earth, which shall be as much superior to man, as man himself is to brutes; or that manlike jelly-fishes do not inhabit Jupiter.

These are all hypotheses, with varying amounts of evidence in their favour. The hypothesis with so much evidence in its favour that it is not possible for an earnest, sensible, and unprejudiced man to refuse to accept it, in his time at least, as true, and the hypothesis in favour of which the evidence is so slight that it cannot but be regarded by thinking men as

I obtained the abnormal cones, and found several similar ones, some of which had withered, while on others the scales had become enlarged, and were approaching maturity. On one cone I found that not only the lowermost scales, but five or six a little higher up, were in the axils of well-developed stamens. Another cone which I examined had a few scales at its base; above this was a staminiferous portion; while further up, and terminating the cone, there were scales again—the whole reminding one somewhat of the inflorescence in *Arum maculatum*.

In another cone (from *Abies nigra*) which I obtained in Perthshire while these pages were in the press, the greater number of the scales upon the lower two-thirds of the cone had their bracts replaced by stamens. The axis of the cone at that part was somewhat elongated, the scales being laxly arranged and not very well developed.

childish, and unworthy to take the place of explanations received by them up to that time as true, are separated from each other by almost insensible gradations. It is clear that in the interval between these two extremes are a number of hypotheses which it will be very difficult to receive or reject; which can only have their true place assigned to them after the evidence has been very deliberately weighed in the most unprejudiced, and painstaking manner.

A theory which is incompatible with views long entertained, and of slow growth, which tends to subvert existing notions, and, indirectly at least, to raise harassing doubts on sacred subjects, should be clearly supported by facts far outweighing those which can be brought forward against it. Otherwise its author, so far from helping the cause of science, and aiding the progress of truth, is retarding real advancement, shaking the faith of those whose disposition is vacillating and undecided, and tends to plunge into the abyss of scepticism those who are so weak or so idle that they would always rather embrace the thoughts of those who will think for them than take the trouble to analyse the facts and opinions on which the conclusions they are called upon to accept are based. I do not advance these statements to prejudice the reader, but simply to show how great an amount of responsibility is taken by the advocate of such a doctrine as the one under consideration.

The theory does not suppose, like some which have preceded it, that species and genera of animals pass, in the course of successive generations, into other forms, but that certain individuals of a race, being exposed to circumstances different to the general mass, will in consequence become somewhat altered. Their habits and instincts are supposed to become modified, and their structure adapted to suit the new conditions under which they exist. If these conditions were unfavourable, they would soon die out; if favourable, the creatures would, in the course of successive generations, undergo still greater modifications, until there was little resemblance to be traced between them and their original progenitors. The latter, in some cases, would disappear altogether, in others would retrograde, and, in some instances, might be supposed to retain for a time their original type, destined perhaps at length to undergo a different order of changes, in consequence of being exposed to different external conditions.

At the same time, to raise this idea above the condition of a mere fanciful speculation, it is necessary to show that creatures exposed to different external conditions do actually undergo change in habits, form, and structure, and the author takes immense pains to prove that his idea is supported by facts, which may now be actually observed in progress; and having shown this modification of structure, habits, and instincts, to his satisfaction, proceeds to argue how greater alterations in conditions would give rise to greater changes in the creatures, and how these acting through vast ages will explain the origin of all the countless varieties of living beings now upon the surface of our earth, or yet to appear. Upon the validity of these facts the probability of the truth of the doctrine mainly rests.

Many believe that the modifications which occur under altered external conditions are more limited in their extent and more subordinate in their nature than the advocates of this theory suppose. That even man is influenced by temperature, food, habits, clothing, no one will deny, but whether these influences, acting through any amount of time, would be capable of producing anything but an altered man, is quite another matter. Wonderful indeed are the differences observed in the physical, mental, and moral conditions of various races of men, and among tribes and individuals of a race; but yet all are men, and distinguished from every other creature by essential differences, infinitely greater than the nonessential and more subordinate characters which distinguish these tribes and individuals from each other.

As man is affected by external conditions, every living thing below him is also affected, but in a very much greater degree. Plants are modified to a far greater extent than animals, animals than man. Varieties are most easily produced in plants, and without great difficulty in some animals. The animals which are most under our influence being those in which the greatest modifications are produced; but these are still only subordinate modifications. Look at the endless varieties of dogs, and the comparatively slight differences observed among successive generations of cats. How soon, too, the cat reassumes its wild state compared with the dog. Yet through all the varieties of dogs, there has never been produced a generation of wolves or foxes. Every variety of dog looked at as a whole, considered with reference to his habits, form, instincts, and his whole being, is still but a dog; so it is with goats, sheep, oxen, pigs, pigeons, fowls, canary birds, &c.

In the lower animals, it is more difficult to point out specific differences than in the higher, in plants more difficult still. The more simple the organised body, the more likely will it be that it will be much altered by external conditions, the more inconstant will be its characters, and the more difficult will it be to assign to it its exact specific peculiarities. It must be borne in mind that, among the higher animals, and even in man, there are certain subordinate points which perhaps may be roughly compared to the colour and form of petals or leaves in plants, which are readily affected by external circumstances. But these changes are, as far as has yet been proved, *limited.* To assign the exact limit in the present state of knowledge is very difficult, and perhaps quite impossible. It may however be remarked, that in the vegetable kingdom it is only certain allied plants that can be propagated by grafting, and among animals there is no race of true hybrids. A plant becomes very greatly modified, an animal in some important particulars, a man in comparatively very slight degree, the alterations affecting only the size of his limbs, colour and texture of his skin, hair, &c.

External circumstances affect different creatures closely allied to each other in different degrees, and a uniform result is not produced by corresponding external conditions. In the language of those who refer all the phenomena of life to physical causes, it requires a much greater amount of heat, food, &c., to develop a goatmoth than a bird—although the structure of the latter is so much more complicated, and its position so much higher in the animal scale than the former. The influence which external conditions exert upon the characters of an animal depends then in great measure upon the nature of its organisation—upon certain individual peculiarities which it has inherited in lineal descent from its original parents.

From all that has been observed, it appears that there are certain peculiarities which do remain fixed and permanent, while there are others which change. Before we can separate the former from the latter class we must be acquainted with the history of the whole life of certain allied forms, from their origin to their death.

Now we are not acquainted with the nature of the principal physical phenomena occurring in the life of one single living being. We have no instrument which will show us the nature of certain physical changes which we know must be taking place even in the simplest organisms—we know not how the creature is produced, how it appropriates material, or how it produces others like itself.

We are overwhelmed with the complicated chemical changes which must take place in the alteration of a little carbonic acid, ammonia, and water, into gum, sugar, albumen, &c.

There is nothing analogous to these changes in the inorganic world. They only occur in living beings—they are constant. They occur in the same order in every individual of a species, but under different circumstances in different species.* The nature of the chemical compounds in animals of the same species is, as far as is known, always the same, but often in very closely allied species very different.

It may therefore be fairly said, that within certain narrow limits,—in the changes occurring in their development, in the structure of their tissues, in the period at which these issues are formed, in the anatomy of their most important internal organs, in the performance of various physiological actions, in the composition and mode of formation of their secretions, in the quantities of these measured in relation to a certain weight of their body, due correction being made for varying amounts of food; in short, in the principal phenomena which are summed up in the history of their lives, as yet but imperfectly investigated,—animals of the same species and varieties resemble each other, while closely allied species differ in very important particulars.

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* This term is only used here in its general sense.

The facts advanced surely speak as much in favour of the permanence of species as do different varieties of dogs, pigeons, chickens, which are after all still but dogs, pigeons, and chickens, for the modification of species.

Regarded then solely from this one point of view, the facts brought forward in favour of the possibility of the *indefinite* modification of animals under the influence of varying external conditions, and, as a consequence, the origin of species by natural selection, do not seem to be so one-sided or so conclusive as we have been led to expect; and, strange to say, the author himself seems to have a doubt if his conclusions are quite justified by the premises, for he frequently tells us there is more evidence which is not brought forward; though it is quite obvious that one strong fact, or series of facts, clearly stated, and to the point, would render quite unnecessary this vast profusion of weak detail, and the discussions to which his book has given rise.

The author's followers are, however, far from sharing any of his doubts or scruples, and appear rather in the light of warm advocates than as unbiassed inquirers into the truth of one of the most difficult problems which man has ever dared to attempt to solve.

That certain modifications may occur by a process of natural selection may be accepted as a very valuable explanation of certain observed facts; but in attempting to expand this into a law of universal application, the author has surely gone too far. One would think that no one can feel more intensely than he the vast and complicated nature of the problem he attempts to solve, and the extreme difficulty of judging correctly of the exact value of every fact presented to him ; and yet, unless the evidence to be produced is far more weighty than that at present brought forward, the arguments in favour of the universal application of the doctrine appear to be far from conclusive.

The difficulty of assigning specific differences in numerous cases is indeed very great, but we must bear in mind that this difficulty arises from the imperfection in the very nature of the existing classification. From the imperfection of their mental power, men were soon compelled to select facts and phenomena of a like kind from the vast multitude which they observed, and to attempt to arrange them in certain groups.

Differences principally of external form were arbitrarily laid down, and these were made to determine the position in which the creature was to be placed. As observations multiplied, creatures were discovered which it was necessary to arrange in new groups, and those groups which agreed in certain general particulars were collected into larger ones, and these again into still larger ones. Travellers soon brought home new forms somewhat resembling those already known, but differing from them in certain well defined characters, and it was necessary to assign to them names by which they might be distinguished from each other, and contrasted or compared. Thus classes, orders, genera, and species were formed.

Increased examination led to the discovery of more peculiarities, and thus every being which differed ever so slightly from another being was to have a distinct name assigned to it. The artificial catalogue, however, was fast becoming useless from its vastness. Creatures which, from great haste, imperfect knowledge, or insufficient powers of observation, had been placed by the naturalist in one group, were, by subsequent and more careful research, proved to belong to another place. After a time, more extended research rendered it necessary again to alter their position in the animal scale.

At length it is asserted that varieties are not to be distinguished from species, or species from each other. Creatures are found which have certain resemblances to two or more genera or classes, and at last so much confusion results, that many are on the point of rejecting altogether the refinements of classification.

Still, does it follow of necessity that we are compelled to receive one of two dogmas which thinking persons would be inclined at once to reject?—1. That every slight variety arises from a distinct creative act; or, 2. that the peculiarities of every living being of necessity result from the action of certain modified external circumstances without the intervention of a superintending Providence.

Lately it has been demonstrated, not in one solitary instance, but in a vast number of cases occurring in different classes of animals, that creatures, so to say, belong to different groups at various periods of their existence, before their specific form is attained. In other words, the offspring of certain creatures are imperfectly formed and unlike their parents. These larval forms are capable of producing larvæ like themselves, or other imperfect forms somewhat differing from their immediate nonsexual parent. This process often goes on to the production of millions of still imperfect creatures, until at last these produce forms totally unlike themselves—perfect, sexual, and with all the characters of the two original parents of this unnumbered progeny.

These millions of creatures result, then, from one single ovum. In many cases, in the different stages through which the larval forms pass, they live in different media, consume different kinds of food, and are exposed to different external circumstances.

But can it be maintained that external agencies are alone concerned in producing this wonderfully exceptional mode of multiplication, which seems so perfect a plan to ensure the development of a vast number of distinct creatures of the same species within an incredibly short space of time, in spite of their being exposed in each phase of existence to many different chances of destruction—can such facts as these be explained except as resulting from infinite wisdom and design?

The difficulties above referred to are surely in great measure a creation of the human mind. The confusion arises from our imperfect knowledge. Why are we to conclude that creatures have been arranged to fit our artificial plan of classification, or that individual creatures have existed with every slight modification of structure in ascending series from the simplest to the highest organism?

It is possible, at least, that there may be many explanations of the observed phenomena which have never yet been thought of by man. Is it not the contrast between the slight knowledge we now possess of natural objects and our former almost complete ignorance, which causes us to put forward every new idea which arises, as an explanation for *all* natural phenomena?

In our rapacity for general conclusions, we are too apt to glance over or not to observe facts which seem to militate against them, and we are in danger of becoming advocates for a crude theory, and of losing the power of observing, and the happiness of contemplating natural objects *as they are.*

Are we yet in a position to generalise on such subjects? Do we know enough of the different creatures around us to enable us to come to any conclusions as to the nature of the changes occurring in their formation as it takes place under our eyes, much less as to their origin?

Will a century of patient investigation enable us to ascertain the mere anatomy of the organisms of these creatures as we see them, much less to demonstrate the changes going on in their bodies during every moment of their lives? Who will write the history of the physical changes occurring in the life of but one living creature from the time of its birth to its death?

Summary.

It is impossible to prove, in the present state of knowledge, that all existing species have not arisen by natural selection, or that many other hypotheses are untenable.

Hypotheses with strong evidence in their favour are separated from hypotheses supported by the slightest possible evidence by insensible gradations.

A theory incompatible with views long entertained should be supported by facts which outweigh those that can be advanced against it.

The Theory:

Its validity depends upon the possibility of unlimited modifications in animals being produced by external conditions.

Modifications not unlimited.

External conditions produce great but limited modifications in plants, important ones in animals, very slight ones in man.

In all organic beings there are subordinate points which are modified by external conditions; but there are also inherent peculiarities independent of these.

There is a limit to the modifications: Only some plants are propagated by grafting, and there is no race of true hybrids.

The external conditions which may be sufficient to "effect" the development of a creature of high organisation are not necessarily sufficient to produce one much lower in the scale.

To define exactly the characters which are capable of being modified, and those which are immutable, requires a thorough knowledge of the history of the life of allied creatures.

Facts speak more for the permanence of species than for their unlimited alteration.

Darwin probably feels that his conclusions are not fully supported by the premises, as he promises to bring forward more facts in support of his doctrine.

The Doctrine is of limited, but not of universal application.

Classification is artificial—Confusion results from attempts to arrange creatures in certain groups:

Two dogmas of which neither is true.

Possibly the true explanation of the observed phenomena has never yet been thought of.

Our knowledge of the physical changes going on in an organism is not yet sufficient to enable us to arrive at any positive conclusions.

We are not yet acquainted with the history of the physical changes occurring during the life of one single being.

On the Vomer in Man and the Mammalia, and on the Sphenoidal Spongy Bones. By JOHN CLELAND, M.D., Demonstrator of Anatomy in the University of Edinburgh.

The remarks which I am about to make will be confined as much as possible to matters of observation. I shall resist the temptation to enter on the question of the constitution of the vomerine segment of the skull, although it is one on which the statements to be made have an important bearing; I shall content myself with exhibiting the relations of this bone in different mammalia, and, founding upon these and on development, shall show how the vomer in man corresponds in its relations to those of other animals, and what is the nature of the sphenoidal spongy bones.

Last autumn, while disarticulating the skull of a lamb, it came prominently under my notice that the central plate of the sphenoid bone adhered without marks of separation to the presphenoid, while the lateral masses of the ethmoid and the vomer formed one other single piece. On further examination I found that in mammalian skulls the formation of one piece by the vomer and lateral masses of the ethmoid was the general rule, and their separation a rare exception. This is a circumstance so easily seen that one would think it could hardly escape the notice of any one in the habit of disarticulating mammalian skulls, yet I can find no description of it by authorities on human and comparative anatomy. It is, however, as we shall see, the most important of all the connections of the vomer, and throws some valuable light on human anatomy. With respect to the other articulations of the vomer, we shall see, that that with the central plate of the ethmoid is by no means a primary one, and that the most constant of those of its inferior margin is that with the intermaxillary bones.

In the *ruminantia* it is a well-developed elongated bone. Let us take that of the lamb as an example. It consists principally of two laminæ united inferiorly so as to form a groove; deepest posteriorly where the laminæ are most developed, and shallowing away to a scooped extremity in front. In this groove lies the cartilaginous septum of the nose, which is continuous behind with the presphenoid bone. The posterior extremity of the vomer is bifid and slightly dilated, as it is in man, and in front of the dilatation the lines of margin begin to approach, and seem as if they would pass directly forwards; but they are almost immediately lost as fissures in two lateral expansions, which, springing from the vomerine laminæ, pass outwards to the outer and back part of the ethmoid, and are continuous with the principal arches of the framework of that bone. On the upper aspect there is a sharp angle between the laminæ that lie against the cartilaginous septum and their lateral expansions, and the former are prolonged in many animals beyond the angle. Where the ethmoid is joined by the ethmo-vomerine lamina—for so we shall call the expansion just described—it forms the upper part of the nasal foramen of the palate bone, in human anatomy called the spheno-palatine foramen. In the lamb there is not much development of the vomer as a mesial plate below the level of the groove. It articulates inferiorly by a rough sutured edge with the superior maxillary bones, and in front of that its scooped anterior extremity lies for about an eighth of an inch or so on the groove formed by the mesial processes of the intermaxillary bones—the universal method of articulation of the mammalian vomer with the intermaxillary bones (fig. 1).

The vomer of the *cat* is proportionally less elongated than that of the sheep, but like it has little development of the mesial plate beneath the vomerine groove. It articulates by a rough surface with the superior maxillary and palate bones, but with the intermaxillary bones by an elongation forward upon them of the laminæ which bound the groove. These laminæ are connected towards their back part with the lateral masses of the ethmoid, exactly as is the case in the sheep; and at the point where the vomer passes into the ethmoid, the latter presents a minute orbital surface, which lies between the two ascending processes of the palate bone, and completes by a point in its inferior margin the almost perfect nasal foramen of that bone. The sphenoid process of the palate-bone lies between the ethmo-vomerine laminæ and the pterygoid bone. The central plate of the ethmoid does not at all touch the vomer in early life, but the cartilaginous septum of the nose passes back beneath it to the presphenoid bone.

It may be mentioned at once that the nasal foramen of the palate bone is completed by the ethmoid in all the animals examined.

The relations of the vomer in the *fox* and the *pig* are the same as in the cat. In the case of the *hedgehog*, as in the sheep, it does not articulate with the palate bones. In the *horse* also it does not articulate with the palate bones. But the superior connections of the vomer in the horse are peculiar, inasmuch as the inferior surface of the leaflets of the ethmoid, instead of lying in contact, as is usual, with the ethmo-vomerine lamina for a considerable extent, is completely floored in by the upper part of the palate bone, which is expanded for that purpose. Even in the horse, however, a slender lamina, immediately in front of the palate bone, and in contact with its nasal foramen, passes downwards and inwards on each side from the framework of the ethmoidal turbinations to the margin of the vomer; but the vomer and it are not anchylosed until other sutures also have begun to be obliterated.

The vomer in the *rodentia* is remarkable in having very little tendency to come in contact with the superior maxillaries. As far as I have observed, it is always continuous with the lateral masses of the ethmoid.

In the skull of the *rabbit* there is only one great anterior palatine foramen; for, although the mesial processes of the intermaxillaries project well backwards, the palate plates of the superior maxillaries do not come far enough forwards to meet them. The vomer does not at all approach the superior maxillaries; its posterior margin terminates inferiorly in a thickened angle, which articulates with the intermaxillaries in such a manner as to make their inferior aspect continuous with the posterior margin of the vomer. In front of this, the laminæ bounding its groove are prolonged on the upper surface of the intermaxillaries, as we have seen in other animals (fig. 4).

In the *porcupine* and *squirrel* the vomer is not in contact with the superior maxillary bones; in the *rat* and the *beaver* it is.

In the *quadrumana* the mesial process of the intermaxillaries is so slightly developed that the anterior extremity of the vomer frequently falls short of it by a slight interval. In monkeys the vomer and orbital plates of the ethmoid are continuous; but in the skull of a young *Chimpanzee* in the University Museum, the arch of bone which unites them is separated at one extremity from the ethmoid by a suture, and at the other only touches the vomer. This piece of bone has all the essential characters of the sphenoidal spongy bones of the human subject.

The vomer and sphenoidal spongy bones in man.—Having found the vomer and lateral masses of the ethmoid so universally connected, we naturally inquire how they are related in man. They are not in contact. Their only connection is that the expanded portion of the vomer which grasps the rostrum lies beneath the sphenoidal spongy bones, and that these articulate with the lateral masses of the ethmoid. Now, seeing that the sphenoidal spongy bones are recognised as ossifications distinct from the sphenoid, I think we have already sufficient evidence to prove that they represent the ethmo-vomerine laminæ, by aid of what we have noticed in the Chimpanzee's skull; for it is impossible to doubt either that the distinct bone which lies between the orbital plates of the ethmoid and the vomer in that skull corresponds to the ethmo-vomerine lamina of other monkeys; or, on the other hand, that it corresponds to the sphenoidal spongy bone in man. But the correspondence becomes much more distinct when we study the early condition of the sphenoidal spongy bones. The most interesting condition of these bones is when, in the skulls of young children, they can be got completely ossified and not yet destroyed by amalgamation with the neighbouring bones. In this state the sphenoidal spongy bone is somewhat of the shape of a hollow pyramid with the apex directed backwards, its inner aspect parallel to its fellow, and its cavity (the first form of the sphenoidal sinus) opening at its base into the nasal cavity in front (fig. 2). This pyramid is constructed by the union of at least three distinct pieces of bone. Firstly, there is an orbital piece, forming a portion of the wall of the orbit between the ethmoid and sphenoid, an element, I believe, in the formation of the orbital wall not hitherto observed. It articulates with the orbital process of the palate bone, and, together with the inferior piece, completes the nasal foramen of the palate-bone, namely, the foramen called spheno-palatine, but which we have seen to be invariably ethmo-palatine in other animals. The superior piece bounds the sphenoidal sinus above and on the inside, and ultimately becomes incorporated with the sphenoid bone. The inferior piece is the largest of the three; it forms the floor of the sphenoidal sinus, and the under half of its opening in front, and includes the greater part of what has hitherto been recognised, and described under the various names, sphenoidal spongy bone, sphenoidal cornu, and bone of Bertin. Its inner margin is joined by the superior piece at an acute angle, and is prolonged downwards and forwards so as to lie edge to edge with the corresponding lamina of the vomer, immediately in front of the thick dilated part of that bone. Beneath and behind is the sphenoidal process of the palate-bone, and behind that is the internal pterygoid process. In man, therefore, as in other mammalia, we find three processes in succession from behind forwards, viz., the pterygoid bone, the sphenoidal process of the palate-bone, and an arch passing from the ethmoid to the vomer, adapted to it edge to edge; and moreover, this arch completes the foramen which divides the ascending part of the palate-bone. It in every respect, therefore, corresponds with the ethmo-vomerine lamina. The reason why the arch formed by the vomer and ethmoid is broken up in the human subject into so many separate pieces is to be sought in the characteristic peculiarities of the human subject, particularly in the very slight development of the organ of smell, and the rapid curvature of the cranio-facial arch. But on this subject I hope to speak more fully on some future opportunity. The inferior edges of the sphenoidal spongy bones, which in childhood lie edge to edge with the vomer, are in the adult state smoothed down to a mere ridge, and considerably separated from the middle line by the expansion of the sphenoidal sinuses.

We have now seen that the relations of the vomer to the lateral masses of the ethmoid in the human subject are essentially the same as in the mammalia generally. In early life the human vomer resembles those of other mammals in form likewise, and seems to be connected in the same manner with the intermaxillary bones. In the skulls of fœtuses and young children the vomer mainly consists of two laminæ extending upwards on the sides of the cartilaginous septum of the nose. The inferior edge exhibits a flat surface with a raphe in the middle line, which articulates with the superior maxillaries proper, *i. e.*, with the part behind the anterior palatine foramen; and which narrows to an edge behind, where it comes in contact with the palate-bones. But this surface ceases abruptly in front, and only the lamina bounding the groove for the cartilage is prolonged on the intermaxillary part of the palate (fig. 3). In the adult state both the scooped projection lying on the intermaxillaries, and the remains of the surface for articulation with the superior maxillaries, can be seen, when the vomer still admits of being accurately disarticulated. But this is not often, as it soon becomes anchylosed with the neighbouring bones; and even when this has not happened, it requires that portions of the other bones be sacrificed for the sake of removing it entire. As the face elongates, the upper part of the vomer undergoes much alteration; not only is there a considerable development of lamina in the mesial plane beneath the groove, but usually the laminæ bounding the groove deviate from the mesial line, and one of them becomes more developed than the other, and is more extensively anchylosed with the central plate of the ethmoid, which, growing downwards, replaces the cartilage between them. In consequence of these changes taking place at a comparatively early period, the specimens which are sold with disarticulated skulls, and from which the descriptions in text-books are drawn up, are seldom complete, and have most frequently more or less of the central plate of the ethmoid adherent to them. Thus the vomer is described as exhibiting at its upper and back part a cul-de-sac for the rostrum. Such a cul-de-sac is often seen, but the central plate of the ethmoid invariably enters into its formation, for it is only the ethmoid, and never the vomer in the slightest degree, which replaces the cartilaginous septum.

Explanation of Plate V.

Fig. 1. The vomer and lateral masses of the ethmoid of a lamb, seen from below. *a*, The inferior margin of the vomer, rough posteriorly, for articulation with the maxillaries, and smooth in front, where it comes in contact with the intermaxillaries; *b b*, the grooves which complete the nasal foraminæ of the palate bones. The spaces between the grooves and the margins of the vomer represent the ethmo-vomerine laminæ, and on the outer aspects of the grooves are the small orbital surfaces of the ethmoid.

Fig. 2. The vomer, ethmoid, sphenoidal spongy bones, and left palate and maxillary bones, from the skull of an infant; seen from behind (slightly enlarged). *a*, Orbital plate of the ethmoid; *b*, posterior extremity of the vomer; *c*, sphenoidal process of the palate bone; *d*, orbital surface of the palate bone, and immediately above it is the orbital portion of the sphenoidal spongy bone. Between the two processes of the palate bone is the spheno-palatine foramen, completed above by the inferior portion of the sphenoidal spongy bone. *e*, The superior portion of the sphenoidal spongy bone.

Fig. 3. Another view taken from the same specimen: *a*, *b*, *c*, The parts of the inferior margin of the vomer for articulation with the palate, maxillary, and intermaxillary bones respectively; *d*, inferior aspect of the sphenoidal

spongy bone; *e*, orbital plate of the ethmoid seen in perspective; *f*, inferior turbinated process of the ethmoid.

***Fig.* 4. Illustrates the articulations of the vomer in the rabbit. Above are the vomer and ethmoid forming one bone. Beneath are the bones of the upper jaw of the left side, and a portion of the intermaxillary bone of the right side adhering to it. *a*, Anterior extremity of the vomer, grooved for the cartilaginous septum of the nose; *b*, the part of the vomer which articulates with *c*, the extremity of the expanded mesial processes of the intermaxillary bones, forming turbinations in connection with Jacobson's organ.**

REVIEWS AND NOTICES OF BOOKS.

The Glaciers of the Alps; being a Narrative of Excursions and Ascents, an Account of the Origin and Phenomena of Glaciers, and an Exposition of the Physical Principles to which they are related. By JOHN TYNDALL, F.R.S., &c. With Illustrations. One Volume 8vo.

The study of geology has many charms. It awakes in the mind of the student the grandest conceptions of immensity and duration, and undertakes to investigate the causes of the most sublime phenomena of nature. In proportion, therefore, as the culture of the imagination becomes more general in those whose taste leads them to the contemplation of nature rather than of humanity, the study of geology becomes more popular. But men of logical habits of thought, and those whose training has been in the exact sciences, when they apply themselves to this science, and would fain enjoy its theories as others do, find for their own misery that it consists in great measure of hypotheses, each having much perhaps in its own favour, yet none so much as to exclude all the others which conflict with it. Hence minds of this order denying themselves to those speculations, whatever their charms, which plunge into the darkness of immensity and duration, and speculate without fear on what happened *then*, have of late years been watching the phenomena and changes which are going on at the earth's surface *now*, hoping thus to construct a science of geology, based on the operation of known laws and on actual observation.

Of these phenomena and changes none are more full of promise than those of the glaciers. The material of which glaciers consist is in fact merely water in different states, and therefore so simple that there is a fair hope of coming to a full understanding of them. That glaciers belong to the domain of geology cannot be doubted. The relation of water to our own organisation, and to changes of state at that particular temperature, which happens to prevail at the surface of this planet, presenting it to us familiarly, both in the solid state, and in a state of fusion, seems indeed to withdraw it from the category of mineral substances. But this is merely a mistake into which our local position as observers has betrayed us. We remember once, not when on our travels among the glaciers of the Alps of Europe, but on the sunny shore of intertropical Asia within a few degrees of the equator, where ice had never been seen before, on presenting to a group of natives a block of this substance, which had been imported from America by the European inhabitants of the place, we were delighted to see how soon these natives were able to refer it to its true category. They were familiar with rock-crystal, and that in large blocks, sometimes in merely vitreous masses, and sometimes crystallized or devil-cut, as they call it. But the transparent solid now presented to them could not be rock-crystal, for it burnt them, as they said, when they touched it. A short experience of the burns it produced, however, soon convinced them that there was no cause for alarm; and a short observation as to what became of a chip when laid in the palm of the hand, together with the application of the tip of the tongue to the liquid which resulted, soon elicited their unanimous vote that it was *water-rock.* Its novelty to them enabled them at once to seize the analogy. Nor should our familiarity with it or its economic relations to us lead us ever to forget, when we are taking a scientific view of it, that water is in reality a simple rock in a state of fusion, and ice the very type of a purely igneous rock. But if so, then are we at once able to pronounce the shibboleth of the glacier question with which our author and his friends find so much difficulty. If so, we are only to expect that an aqueo-icy mass, a glacier, like products of fusion in general, as they pass slowly from the solid to the liquid state, or from the liquid to the solid, or play lazily between them, or hang permanently on the confines of both, or consist of the one or the other in adjacent points of the interior, shall exhibit a certain indeterminate or amorphous and defective mobility of particles or imperfect fluidity, which, when viewed in the abstract, or apart from forces actually causing it to manifest motion, is expressed by the term *plastic*, but which, when actually and permanently undergoing change, and manifesting that mobility, especially if the surface tends to be adhesive, is more aptly expressed by the term *viscous*. It unfortunately happens, indeed, for the immediate acceptance of this term in the connection in which we have now given it, that most of the viscous substances which are most familiar—treacle or tar, for instance—are also dirty, that is, they are not only adhesive to the object which touches them, but they leave a part of themselves upon it, rendering it nasty, while nothing of this kind is to be apprehended from coming in contact with water-rock, though the wet glove, or the sole of the shoe, as well as the surface of the rock itself, be of such a temperature that they are most favourably related for union. But still no finer conception can be formed of that superficial adhesiveness, which, in popular conception at least, attaches to the idea of viscosity, than that fixing by regelation, which, in the circumstances described, takes place between ice and any moist ice-cold body which touches it. Let it not be said that the term viscous is wholly misapplied unless the substance so characterised be ropy, as well as semi-fluid pasty and adhesive, so as to prove itself ductile when tension is applied to it, and all lateral pressures withdrawn. This is a limitation of the term, which scientific usage does not sanction. When ductility manifests itself, this term is ready to express the fact. But a substance may be such, that no other term in our language is more appropriate to indicate its condition as to solidity and fluidity than the term viscous, though it be not ropy, except when pressures are applied laterally, as well as tension at its extremities. And, indeed, tenacity among the particles of bodies from which ductility results, is a property so peculiar, that there is reason to suspect that it has often been attributed to substances which do not really possess it. Were all those bodies which are usually described as viscid tested as to their ductility, when all lateral pressures are removed—in the exhausted receiver of an air-pump, for instance—it might be found that while the term viscid could not thereafter be denied them, still but a few of them were really ductile in any notable degree. Shall we then refuse to allow the term viscous to be used, unless the viscous substance be also obviously ductile? Take the case of sugar undergoing the process of refining, and now in a semi-fluid state in the vacuum pan. The refiner takes out a sample to test it as to this very property. It is as yet not at all ductile. Placed between the opening finger and thumb it breaks quite short, and held up to the light, the original granular structure is still seen to prevail, though all is already fused into what is assuredly a viscous mass. By and by another sample and another is taken out and tested in the same way; and at last it is found to be uniformly translucent and ropy in a high degree. From the first it was viscous, but now it is ductile also. And to superadd this ductility to the viscosity which results as soon as fusion commences, is all that the refiner aims at in the vacuum-pan. In a similar, and as we think correct, use of language, we find in the best and most recent system of chemistry which our language possesses, the author (Professor Millar), when treating of sulphur, expressing himself thus:—"At 350° it gradually becomes more and more *viscid;* the temperature at this point for a while becomes stationary, notwithstanding continued accessions of heat from without, so that heat is becoming latent," *as in the analagous case of melting ice* (italics not the author's). He then describes a further process, by which it may be transformed into what he designates "*Ductile Sulphur.*"

Such, we conceive, is correct language in reference to the term viscous. The relations between viscosity and ductility are certainly intimate, but they are not necessary, nor does the one property imply the other. And had it not been for a certain weakness in the region of his affections, under which our author manifestly labours when the shadow of Professor Forbes crosses his path, we should certainly never have found such a master of language as he is substituting the term viscosity for ductility, as he does in the following words, which he gives with all the formality of a definition:—"Viscosity, then, consists in the power of being drawn out when subjected to a force of tension, the substance after stretching being in a state of molecular equilibrium, or, in other words, devoid of that elasticity which would restore it to its original form." (P. 312.)

Doubtless it is among the triumphs of geology to have led captive a cultivator of the exact sciences so favourably known as Professor Tyndall. Nor is he himself unaware how much this science has to gain by the devotion of such men as himself and others to be found about the Royal Institution. It may be questioned, however, whether he, or any body else, is right in treating slightingly the labours of naturalists. The exact sciences by themselves —mathematics, both pure and applied—are not an adequate culture, either for the head or the heart. The pursuits of the naturalist are, we think, better for both. Without the cultivation of the powers of observation, which is the naturalist's calling, but little can be done for the correct interpretation of nature. If to this there be added the power of treating these observations mathematically, without trimming them for the purpose, there is great gain no doubt; but we are jealous of all disparagements of naturalists, and we question whether our author is happy in the manner in which he speaks of the admirable author of the "Systéme Glaciaire," when he first introduces him to his readers. "M. Agassiz," says he, "is a naturalist, and he appears to have devoted but little attention to the study of physics. At all events, the physical portions of his writings appear to me to be very often defective. It was probably his own consciousness of this deficiency that led him to invoke the advice of Arago and others previous to setting out upon his excursions. It was also his desire 'to see a philosopher so justly celebrated occupy himself with the subject' which induced him to invite Professor J. D. Forbes of Edinburgh to be his guest upon the Aar in 1841." Now it is plainly possible that the warm-hearted naturalist may have had other motives than a "consciousness of his own deficiency" for taking these steps. But it is possible also that such a style of writing may have a different effect upon our author's mind from what it has upon the reader's. For, at the close of the very section in which he speaks of Agassiz in these terms, he says, "I hope that no expression shall escape me inconsistent with the courtesy which ought to be habitual among philosophers, or with the frank recognition of the just claims of my predecessors" (p. 274). And yet is there not something ominous in these words? Is there to be nothing more cordial and earnest between philosophers than courtesy, even when they are marching together over the same, and these the most rugged tracts of research, and labouring each as best he can to compel Nature to confess her secrets to human intelligence, and to carry on her bosom the impress of humanity?* Yes, truly; and though our author promises no more, yet he gives it. Witness the following spirited extract from his first ascent of Mont Blanc, where his exquisite use of the pronoun of the first person is so finely spiced with sympathy, friendly feeling, and admiring regard for his friend and colleague, Professor Huxley. "While we were away, Huxley sat down upon the ice with an expression of fatigue upon his countenance: the spirit and the muscles were evidently at war, and the resolute will mixed itself strangely with the sense of peril and the feeling of exhaustion. He had been only two days with us, and though his strength is great, he had had no opportunity of hardening himself by previous exercise upon the ice for the task which he had undertaken. The ladder now arrived, and we crossed the crevasse. I was intentionally the last of the party, Huxley being immediately in front of me. The determination of the man disguised his real condition from everybody but myself, but I saw that the exhausting journey over the boulders and debris had been too much for his London limbs. Converting my waterproof haversack into a cushion, I made him sit down upon it at intervals, and by thus breaking the steep ascent into short stages, we reached the cabin of the Grand Mulets together. Here I spread a rug on the boards, and placing my bag for a pillow he lay down, and after an hour's profound sleep he rose refreshed and well; but still he thought it not wise to attempt the ascent farther" (p. 71). Now, what though our author gives his *compagnon de voyage* thus a slap before the public, and leaves him with the night, the icy solitudes of the Mer de Glace, and seventeen long hours as his only companions. Plainly if there be something less, there is also something more than courtesy here. For our own parts, in contemplating the happy meeting of the associated professors again on our author's descent from Mont Blanc, we cannot but join our benediction too, in the words of the philosopher who exclaims, "A blessing on the man that first invented sleep!" What if, instead of one hour having proved sufficient to redintegrate Huxley, he had fallen asleep again in his solitude of icy cold: might not his descending friend have found him "sleeping that sleep that knows no waking," and the Man of Anak whom the Bishop of Oxford encountered this year, so much for his own misery, been merely a biography since 1857? Courtesy is all very well where nothing better is possible; but it always reminds us of a retort which a Cavalier gave to a Roundhead one day when they found themselves at table together, with a tankard of foaming beer between them. "That stout," said the Roundhead, "is like your religion—all show and no substance." "No," rejoined the Cavalier, "it is like you and yours—smile in your face and cut your throat." By all means let us have something warmer and more genuine between philosophers than merely courtesy, else courtesy itself is sure to fail.

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* In virtue of the visits of students of nature, there are now "hotels" and cabins high among the solitudes of the ice, where no shepherd or chamois-hunter would think of placing a chalet.

Our author prefaces his work by an account of its origin; and thus we learn that it took its rise in a lecture that he gave on a "Comparative View of the Cleavage of Crystals and Slate-rocks," delivered at the Royal Institution on Friday the 6th (App.), or 10th (Preface), June 1856. This lecture is given as an appendix to the volume under review, and is very valuable, not only on account of its matter, but also its manner. It is in fact very clever, and quite a type of those most interesting lectures which are given from to time in Albemarle Street to elegant audiences, both adult and juvenile, by the younger fraternity of lecturers attached to the Royal Institution. On this occasion, Professor Sedgwick is the great man who is shown to be in the wrong, and our author the (of course) greater man who sets him right. Professor Sedgwick was however quite innocent as a theorist, when in 1835 he maintained that "no retreat of parts, no contraction of dimensions in passing to a solid state, can explain the phenomena of slate-cleavage, and that they appear resolvable only on the supposition that crystalline or polar forces acted upon the whole mass simultaneously in one direction and with adequate force." This view is an integral part of the Wernerian geology. It was taught in our university by Professor Jameson from the first years of this century, and was published in his "System of Mineralogy" in 1808. Werner's pupils looked upon the whole planet as one grand crystal, and were prepared to find evidence of the action of polar forces among the molecules everywhere—a massive dodecahedral garnet, if we remember rightly, having been adduced in illustration of the conception. Our author explains the phenomenon of slatiness or lamination by showing that compression develops it where it did not exist before. And this experiment he regards as excluding the more recondite conception of Werner, Jameson, and Sedgwick, of polar forces and a nisus at crystallisation. But the two hypotheses do not conflict. Perhaps compression develops a lamellar structure only by rendering possible, or establishing by the motion which accompanies it such positions in the molecules constituting the mass, as bring their polar forces into parallel play. But what, it may be asked, had a lecture on the slaty structure to do with glaciers? To this it is to be answered, that the ice of glaciers in many places displays a ribbon structure of alternately blue and white ice, ice without, and ice with, manifold air bubbles in it, thus simulating lamination. Now, it happens that on this subject there are to be found many observations and speculations in Professor Forbes' "Travels among the Alps." With this work our author informs us that Professor Huxley, who was present at the lecture, was well acquainted, and he surmised that the question of slaty cleavage, in its new aspect, might have some bearing upon the laminated structure of glacier-ice discussed, in the work referred to. "He therefore urged me," continues our author, "to read the "Travels," which I did with care, and the book made the same impression upon me that it had produced upon my friend" (p. 7). And shortly after they set out to Switzerland together. Thus it is to Professor Forbes' previous labours, and his great work on the Alps, that we owe those of Professor Tyndall and the very clever volume now under review. What the impression which the perusal of the "Travels" produced upon Professor Tyndall and his friend he does not inform us; but let us hope, in the meantime, that it was very favourable, since that is the general impression which the perusal of that volume produces, and since Professor Forbes is a universally esteemed philosopher, who, to qualify himself for a thorough study of glaciers, had, previously to the publication of his volume in 1843, crossed the principal chain of Alps twenty-seven times, generally on foot, by twenty-three different passes, and had of course intersected the lateral chains in very many directions,* and has taught at least one lesson for which such a spirited and adventurous iceman as Professor Tyndall ought to be thankful—viz., that it is possible to injure one's health permanently by over-exposure and over-fatigue in endeavouring to interpret the phenomena of icy mountains.

Thus we find our author among the Alps, and certainly nothing can surpass the spirit and life with which he both acts on the mountain and writes in his book. His excursions generally, and especially his first ascent of Mont Blanc and that of the Finsteraarhorn, are beautifully described and extremely interesting; and he actually makes his reader tremble for his safety when he ventures on the ascent of Monte Rosa alone. But he accomplished it, and with such expedition that, but for a voluntary delay on overtaking another party, when beneath the region of danger, the time of the achievement would only have been nine hours!—about half that usually allowed. His powers are admirable. In fact, he sticks at nothing. And to show that exact science has in no degree banished the genius of poetry from his soul, take the following paragraph from the ascent of Finsteraarhorn, which is one of scores as finely done:—"Two hours' walking brought us to our place of rest; the porters had already reached it, and were now returning. We deviated to the right, and having crossed some ice ravines, reached the lateral moraine of the glacier, and picked our way between it and the adjacent mountain wall. We then reached a kind of amphitheatre, crossed it, and climbing the opposite slope, came to a triple grotto formed by clefts in the mountain. In one of these a pine fire was soon blazing briskly, and casting its red light on the surrounding objects, though but half dispelling the gloom from the deeper portions of the cell. I left the grotto, and climbed the rocks above it to look at the heavens. The sun had quitted our firmament, but still tinted the clouds with red and purple; while one peak of snow, in particular, glowed like fire, so vivid was its illumination. During our journey upwards, the Jungfrau never once showed her head, but, as if in ill temper, had wrapped her vapoury veil around her. She now looked more good-humoured, but still she did not quite remove her hood, though all the other summits, without a trace of cloud to mask their beautiful forms, pointed heavenward. The calmness was perfect—no sound of living creature, no whisper of a breeze, no gurgle of water, no rustle of debris, to break the deep and solemn silence. Surely if beauty be an object of worship, those glorious mountains, with rounded shoulders of purest white—snow-crested and star-gemmed—were well calculated to excite sentiments of adoration." Our author does not say that they actually did produce sentiments of adoration in himself, and we are very far from accusing him of idolatry; but we think it would have enhanced the value of his work did we find him, in scenes of such grandeur and of danger as he so often faced, giving utterance somewhere at least to those feelings of religious emotion which are of the very essence of humanity, but of which we seek in vain for one trace in the entire volume.

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* Forbes' "Travels through the Alps," &c. p. 10.

To the narrative of his various expeditions among the Alps, including, among others, two ascents of Mont Blanc and of Monte Rosa, and a winter expedition to the Mer de Glace, the first half of his volume is devoted. The second treats scientifically of the phenomena of glaciers, and this part is introduced by some admirable paragraphs on light and heat, in virtue of which any reader of a fair general education will be enabled to understand what follows. There is also a chapter on "Heat and Work," in which the most modern ideas of their mutual convertibility are maintained and felicitously explained. In due course our author comes to the grand subject of glacier motion, and records the many accurate observations and measurements which he has made upon it, both in several summers and in last winter. He then gives a summary of the various theories which have been proposed to account for it, and comments dashingly upon them. But here he breaks strangely down. What our author has done, and done admirably well, is to show, by observation and experiment, by accurate conception and discussion, that the views of the brothers Professors J. and W. Thomson, of Faraday, and Sorby, on liquefaction, regelation, compression, and lamination, in sequence of those of Hall, Rendu, and Forbes on glaciers, and the theory of the last (which is, that "a glacier is an imperfect fluid, or a viscous body, which is urged down slopes of a certain inclination by the mutual pressure of its parts"), are in great measure correct, and adequate to account for the phenomena of glaciers. But instead of fully assenting to any of them, or adducing his own views as verifications, he writes as if bent on superseding their labours by his own, with the exception of those of Rendu. And with regard to Forbes, in particular, his aim seems to be to reduce to a minimum everything that he has done. Our author, in fact, presents himself to us as a supreme discoverer in the theory of glaciers. And thus it is that he perplexes the reader; while, for our own part, we must confess that he vexes us also; since he will not allow us to respect any longer, as we have been accustomed to do, those to whom we may have been hitherto thankful for the light they have thrown upon Nature's footsteps in the snowy mountains. Sometimes, indeed, he places matters on a better footing, and to such passages we gladly turn. When closing his narrative of observations on the motion of glaciers, for instance, he says: "To sum up this part of the question,—the *idea* of semi-fluid motion belongs to Rendu; the *proof* of the quicker central flow belongs in part to Rendu, but almost wholly to Agassiz and Forbes; the proof of the retardation of the bed belongs to Forbes alone; while the discovery of the locus of the point of maximum motion belongs, I suppose, to me." (P. 310.) The discovery which he here so modestly refers to himself is a very beautiful one. It is indeed necessarily included in the general theory, as suggested by Rendu, and proved by Forbes, independently and within a short time of each other, now nearly twenty years ago, and first advanced in this country by the latter in this Journal. But still our author's observations were needed. Agassiz had previously observed that the maximum downward motion of the glacier was not always in the axis of the valley or middle of the glacier, but deviated considerably to the right at one place, to the left at another. Our author made accurate measurements on the subject, and connected geometrically the deviation of the maximum from side to side with the salient and re-entrant angles of the valley which formed the bed of the glacier, thus showing that "its analogy with a river is complete."

But the occurrence of the name of Rendu in the above extract calls upon us to signalise a peculiarity of the work under review —so admirable in many ways—with respect to which we cannot but say, that its author is deserving of unmitigated censure. It is impossible to read his section on Rendu's Theory (pp. 299-308) without feeling obliged to conclude that he insinuates a moral obliquity on the part of Professor Forbes in connection with that theory,—in fact a concealment of passages in the essay of the Savoy Bishop, with a view to his own aggrandisement as a theorist, if not as a discoverer also. For our own part, on perusing "The Travels among the Alps," our feeling was that Forbes gloried in Rendu's views, and lost no opportunity in bringing them favourably forward as anticipations of his own. Thus to his all-important Chapter XXI., "An Attempt to Explain the Leading Phenomena of Glaciers," he gives a long extract from Rendu's Essay, even as the motto of the chapter. At p. 367 he says, "When a glacier passes from a narrow gorge into a wide valley, it spreads itself, in accommodation to its new circumstances, as a viscous substance would do; and when embayed between rocks, it finds its outlet through a narrower channel than that by which it entered. This remarkable feature of glacier motion, already several times adverted to, had not been brought prominently forward until stated by M. Rendu, now Bishop of Annécy, who has described it very clearly in these words." Then follows the extract, and in connection with it, in a foot-note, he says—"Whilst I am anxious to show how far the sagacious views of M. Rendu coincide with, as they also preceded, my own, it is fair to mention, that all my experiments were made, and indeed by far the greater portion of the present volume was written, before I succeeded in obtaining access to M. Rendu's work, in the tenth volume of the Memoirs of the Academy of Chambery, which I owe at length to the kindness of the Right Reverend author." No one who knows Professor Forbes, either personally or in his philosophical career and character, or who reads with candour his "Travels," will doubt that what he here states is a true expression of the state of his mind. But it is equally impossible to doubt that the impression which the section of the work under review is calculated to produce upon the reader, is that Forbes has not handled Rendu fairly. Our author admits that in Professor Forbes' writings there are "frequent and flattering references" to Rendu. But he adds, that "there are others of much greater importance which have hitherto remained unknown in this country" (p. 303). Passages which are no doubt intended to support this character of them he then quotes; and in a footnote at the end of the section he says, "In all that has been written upon glaciers in this country, the above passages from the writings of Rendu are unquoted; and many who mingled very warmly in the discussions of the subject were, until quite recently, ignorant of their existence. I was long in this condition myself; for I never supposed that passages which bear so directly upon a point so much discussed, and of such cardinal import, could have been overlooked; or that the task of calling attention to them should devolve upon myself nearly twenty years after their publication. Now that they are discovered, I conceive no difference of opinion can exist as to the propriety of placing them in their true position" (p. 308). Bravo! but where are they? We profess that the conception which we have of Rendu's essay from Forbes' "Travels," as well as the extracts which Forbes gives, are more complete than anything that we find in this volume. The extracts which our author gives are few compared with those which Forbes gives, and of comparatively little value. Still we are not sorry at having placed before our readers the fine flourish of trumpets by which our author accompanies them. It is a model of composition in its way, and a fine illustration how to put a mountain in labour majestically. And yet we must confess that the "mus" in which it has its issue is not simply "ridiculus." Professor Forbes has been selected by the editors of our most authoritative Encyclopædia as the historian of the natural philosophy of the last fifty years. And the history which he has composed, and which has been published in his name, both in the great work and separately, is extensively read, and has indeed attained to the position of a work of reference so far as it goes. But if we accept Professor Tyndall's views of his treatment of the Bishop of Annécy, he is plainly unworthy of credit. We are therefore not surprised to see that he has felt called upon to justify himself from our author's insinuations, and has just published something on the subject.* We need therefore say no more.

And to come to a more grateful theme, we would now remark, that our author, after very interesting chapters on the pressure of ice, in which he shows how it can be formed in this way even into statuettes; on a very interesting phenomenon, happily designated by him regelation; on the "nursing" of snow-crystals and the dissection of ice by a sunbeam, proceeds, at the close of his volume, to give what he designates a "partial summary," but which in reality, within the compass of four pages, and no more than four-and-twenty short paragraphs, is a distinct account of glaciers and their phenomena. We think that we cannot do better than present this to our readers in its integrity.

"1. Glaciers are derived from mountain snow, which has been consolidated to ice by pressure.

2. That pressure is competent to convert snow into ice, has been proved by experiment.

3. The power of yielding to pressure diminishes as the mass becomes more compact; but it does not cease, even when the substance has attained the compactness which would entitle it to be called ice.

4. When a sufficient depth of such a substance collects upon the earth's surface, the lower portions are squeezed out by the pressure of the superincumbent mass. If it rests upon a slope, it will yield principally in the direction of the slope, and move downwards.

5. In addition to this, the whole mass slides bodily along its inclined bed, and leaves the traces of its sliding on the rocks over which it passes, grinding off their asperities, and marking them with grooves and scratches in the direction of the motion.

6. In this way the deposit of consolidated and unconsolidated snow which covers the higher portions of lofty mountains, moves slowly down into an adjacent valley, through which it descends

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* Reply to Professor Tyndall's Remarks, &c., by James David Forbes, D.C.L., &c. Pp. 28. Adam & Charles Black, Edinburgh. 1860.

as a true glacier, partly by sliding and partly by the yielding of the mass itself.

7. Several valleys thus filled may unite into a single valley, the tributary glaciers welding themselves together to form a trunk glacier.

8. Both the main valley and its tributaries are often sinuous, and the tributaries must change their direction to form the trunk. The width of the valley often varies. The glacier is forced through narrow gorges, widening after it has passed them. The centre of the glacier moves more quickly than the sides, and the surface more quickly than the bottom. The point of swiftest motion follows the same law as that observed in the flow of rivers, shifting from one side of the centre to the other as the flexure of the valley changes.

9. These various effects may be reproduced by experiments on small masses of ice. The substance may, moreover, be moulded into vases and statuettes. Straight bars of it may be bent into rings, or even coiled into busts.

10. Ice capable of being thus moulded is practically incapable of being stretched. The condition essential to success is, that the particles of ice operated on shall be kept in close contact, so that when old attachments have been severed new ones may be established.

11. The nearer the ice is to its melting-point in temperature, the more easily are the above results obtained. When ice is many degrees below its freezing point, it is crushed by pressure to a white powder, and is not capable of being moulded as above.

12. Two pieces of ice at 32° Fahr., with moist surfaces, when placed in contact, freeze together to a rigid mass. This is called regelation.

13. When the attachments of pressed ice are broken, the continuity of the mass is restored by the regelation of the new contiguous surfaces. Regelation, also, enables two tributary glaciers to weld themselves to form a continuous trunk; thus, also, the crevasses are mended, and the dislocations of the glacier consequent on descending cascades are repaired. This healing of ruptures extends to the smallest particles of the mass, and it enables us to account for the continued compactness of the ice during the descent of the glacier.

14. The quality of viscosity is practically absent in glacier-ice. Where pressure comes into play, the phenomena are suggestive

of viscosity; but where tension comes into play, the analogy with a viscous body breaks down. When subjected to strain, the glacier does not yield by stretching but by breaking. This is the origin of the crevasses.

15. The crevasses are produced by the mechanical strains to which the glacier is subjected. They are divided into marginal, transverse, and longitudinal crevasses: the first produced by the oblique strain consequent on the quicker motion of the centre; the second, by the passage of the glacier over the summit of an incline; the third, by pressure from behind and resistance in front, which causes the mass to split at right angles to the pressure.

16. The moulins are formed by deep cracks intersecting glacier rivulets. The water, in descending such cracks, scoops out for itself a shaft, sometimes many feet wide, and some hundreds of feet deep, into which the cataract plunges with a sound like thunder. The supply of water is periodically cut off from the moulins by fresh cracks, in which new moulins are formed.

17. The lateral moraines are formed from the debris which loads the glacier along its edges; the medial moraines are formed on a trunk-glacier by the union of the lateral moraines of its tributaries; the terminal moraines are formed from the debris carried by the glacier to its terminus, and there deposited. The number of medial moraines on a trunk-glacier is always one less than the number of tributaries.

18. When ordinary lake-ice is intersected by a strong sunbeam, it liquefies so as to form flower-shaped figures within the mass; each flower consists of six petals, with a vacuous space at the centre; the flowers are always formed parallel to the planes of freezing, and depend on the crystallisation of the substance.

19. Innumerable liquid disks, with vacuous spots, are also formed by the solar beams in glacier ice. These empty spaces have been hitherto mistaken for air-bubbles, the flat form of the disks being erroneously regarded as the result of pressure.

20. These disks are indicators of the intimate constitution of glacier ice, and they teach us that it is composed of an aggregate of parts, with surfaces of crystallisation in all possible planes.

21. There are also innumerable small cells in glacier-ice holding air and water; such cells also occur in lake-ice, and here they are due to the melting of the ice in contact with the bubble of air. Experiments are needed on glacier-ice in reference to this point.

22. At a free surface within or without, ice melts with more ease than in the centre of a compact mass. The motion which we call heat is less controlled at a free surface, and it liberates the molecules from the solid condition sooner than when the atoms are surrounded on all sides by other atoms which impede the molecular motion. Regelation is the complementary effect to the above, for here the superficial portions of a mass of ice are made virtually central of a second mass.

23. The dirt-bands have their origin in the ice-cascades. The glacier, in passing the brow, is transversely fractured; ridges are formed with hollows between them; these transverse hollows are the principal receptacles of the fine debris scattered over the glacier; and after the ridges have been melted away, the dirt remains in successive stripes upon the glacier.

24. The ice of many glaciers is laminated, and when weathered may be cloven into thin plates. In the sound ice, the lamination manifests itself in blue stripes drawn through the general whitish mass of the glacier; these blue veins representing portions of ice from which the air-bubbles have been more completely expelled. This is the veined structure of the ice. It is divided into marginal and longitudinal structure, which may be regarded as complementary to marginal, longitudinal, and transverse crevasses. The latter are produced by tension, the former by pressure, which acts in two different ways: *firstly*, the pressure acts upon the ice as it has acted upon rocks which exhibit the lamination technically called cleavage; *secondly*, it produces partial liquefaction of the ice. The liquid spaces thus formed help the escape of the air from the glacier; and the water produced, being refrozen when the pressure is relieved, helps to form the blue veins."

Such is the summary of what our author believes or finds with regard to glaciers; and as a summary of our knowledge of their phenomena, every reader must appreciate its excellence. But when viewed in the light of discovery, we confess that we do not find room for saying more than has been said in this Journal already, eighteen months ago before the volume now in hand was published. It then fell to us to notice Professor Forbes' "Occasional Papers on the Theory of Glaciers," published last year. And as Professor Tyndall's contributions on the compression of ice, its regelation, and its six-petal flowers, had been published in various quarters previously to this date, though only now brought together in his volume on "The Glaciers of the

Alps," the author of the "Occasional Papers" had an opportunity of considering their bearing upon his own researches and his theory of glaciers. The result of that consideration he gives in the preface of the volume referred to; and to us his estimate seemed so just, and so much in the interest of scientific progress, that we quoted a considerable portion of it, which is accordingly to be found in our pages.* Nor, after the perusal of the volume which we now close with regret, do we find any occasion to modify the estimate there made. We only wish that the honouring language and ingenuous feelings with which Principal Forbes refers to Professor Tyndall in the work just quoted had been sincerely reciprocated by the latter; and "The Glaciers of the Alps," as it is one of the cleverest, so would it have been one of the best excursion books and scientific manuals in our language.

PROCEEDINGS OF SOCIETIES.

British Association for the Advancement of Science.

Meeting at Oxford, Wednesday 26th June to Wednesday 3d July 1860.

The annual meeting of the British Association this year, under the presidency of Lord Wrottesley, took place at Oxford on the 26th of June. At that season the Scottish Universities are in the midst of the summer session, which prevented many men of science in this country from being present.

As usual, it was in the Sections chiefly that any contributions to science were made, and of the most important of them we now proceed to give an abstract:—

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

The President of this Section, the Rev. B. PRICE, opened it by an able address in the interest of mathematical science and British mathematicians, declining to accede to the disparagement of mathematical evidence which certain foreign mathematicians have of late been insisting upon, and proposing that in the papers to be brought before the Section those in pure mathematics should take the precedence, to be followed next by those which belong to applied mathematics, and afterwards by those who do not admit of mathematical terms and symbols. This proposal, however, went for nothing in what followed. The number of papers offered was indeed so great that a sub-Section had to be formed in order to receive a portion of them. But in the review of the whole, there are only a few which possess more than the transient interest of the hour. Among those which possess permanent interest, the first that appears is an attempt by Mr J. Brown to estimate the velocity of earthquake shocks in the laterite of India, a clayey rock in a semi-pasty condition of perhaps the lowest degree of elasticity, reposing in some places on strata of sand and clay. Supposing the shock to have travelled from Quelon to Trevandrum, and taking the distance between these two places at thirty-seven miles, a velocity of propagation is obtained at between 470 and 530 feet per second, according to the time marked by different observers.

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* See "Edinburgh New Philosophical Journal," New Series, vol. ix., April 1859, p. 277.

The same day two communications were made by M. Claudet, of which the former had for object to show that in order to obtain a perfectly good picture of any size from a small negative,—in order to obtain a portrait, for instance, the size of life from a miniature the size of a visiting card,—all that is necessary is to have recourse to Woodward's solar camera, provided it be accurately made, and so adjusted that the focus of the condensing lens fall exactly on the front lens of the camera obscura, neither behind it nor before it, as is common in the instruments as usually made. M. Claudet's other communication related to the means of retaining stereoscopic effect along with magnifying power, which he proposed to effect by prisms.

The following day the Section received a valuable communication from Admiral FITZROY *on British Storms*, in which he entered into many details of recent storms, and concluded with the following interesting intelligence:—The British Association has made application to Her Majesty's Government to authorise arrangements for communicating warning of storms from one part of the country to the other; and, in conclusion, I will read to you the details of that arrangement which promises to be so beneficial. Arrangements have been authorised by the Board of Trade (under a minute from the President, dated June 6), in consequence of which a daily and mutual interchange of certain limited meteorological information will be transmitted between London and Paris, the results of five subsidiary communications to the central stations of Paris and London. Authority being thus given to collect and communicate, by the telegraph, particular meteorological intelligence, a commencement may be made on the 1st of September, as the plan proposed is simple and the machinery is ready. Once a day, at about nine A.M., barometer and thermometer heights, state of weather and direction of wind, will be telegraphed to London from the most distant ends of our longest wires,—namely, Aberdeen, Berwick, Hull, Yarmouth, Dover, Portsmouth, Jersey, Plymouth, Penzance, Cork, Galway, Londonderry, and Greenock. Facts sent thus from five of these places will be put into one telegram and sent to Paris immediately, when a corresponding communication will be made from the Atlantic coasts southward. When threatening signs are not apparent, no further notice will be transmitted to or from London on that day, respecting weather. But when indications are such as to warrant some cautionary signal at a certain part of, or along all our coasts, the words "Caution,—North" (or "South") will be sent to some of the thirteen places specified, or to all of them; on the receipt of which a cone (or triangle) will be hoisted at a staff (point up for north, down for south), indicating the side whence wind may be expected. This signal will be repeated along part of the coast by the coast-guard, at such of their stations as may be authorised (at most of their stations, flagstaffs are visible to coasters). Danger will be implied by a drum (or square), a cone, and perhaps, in addition, very great danger by a cone, a drum, and a second cone. (The cones and drums may be made with hoops and black canvas, to collapse, without top or bottom. They will be the same shape from all points of view, and unlike any other signal, such as a time-ball, used ordinarily.) As the coast-guard extends all along the frequented parts of our shores, and as the telegraph companies are liberally willing to have instruments and signals placed at their extreme stations, in charge of and used by their officials, only the necessary materials and instructions will be required, all of which are ready or in progress. By vigilance at the central station, and by taking great care to avoid signalling too frequently, much may be done towards diminishing the losses of life on our increasingly crowded coasts.

On Saturday, Sir D. Brewster read a paper *on Microscopic Vision and a New Form of the Microscope.* In this the worthy Principal again, after an interval of more than a quarter of a century, and notwithstanding all the disappointments which have intervened, recommends gems as material for lenses instead of glass. He objects even on the ground of truthfulness to object-glasses with large apertures, and sums up thus the other improvements which he suggests:—1. The first step, we conceive, is, to abandon large angular apertures, and to use object-glasses of moderate focal length, obtaining at the eye-glass any additional magnifying power that may be required. 2. In order to obtain a better illumination, either by light incident vertically or obliquely, a new form of the microscope would be advantageous. In place of directing the microscope to the object itself, placed as it now is almost touching the object-glass, let it be directed to an image of the object, formed by the thinnest achromatic lens, of such a focal length that the object may be an inch or more from the lens, and its image equal to, or greater or less than the object. In this way the observer will be able to illuminate the object, whether opaque or transparent, and may subject it to any experiments he may desire to make upon it. It may thus be studied without a covering of glass, and when its parts are developed by immersion in a fluid. 3. The sources of error arising from the want of perfect polish and perfect homogeneity of the glass of which the lenses are composed, are, to some extent, hypothetical; but there are reasons for believing,—and these reasons corroborated by facts—that a body whose ingredients are united by fusion, and kept in a state of constraint from which they are striving to get free, cannot possess that homogeneity of structure, or that perfection of polish, which will allow the rays of light to be refracted and transmitted without injurious modification. If glass is to be used for the lenses of microscopes, long and careful annealing should be adopted, and the polishing process should be continued long after it appears perfect to the optician. We believe, however, that the time is not distant when transparent minerals, in which their elements are united in definite proportions, will be substituted for glass. Diamond, topaz, and rock-crystal are those which appear best suited for lenses. The white topaz of New Holland is particularly fitted for optical purposes, as its double refractions may be removed by cutting it in plates perpendicular to one of its optical axes. In rock-crystal the structure is, generally speaking, less perfect along the axis of double refraction than in any other direction, but this imperfection does not exist in topaz.

On a following day, Sir David resumed his observations on the optical phenomena of decomposed glass, which he has found to be so interesting. In addition to the phenomena in polarisation which he formerly observed, he now finds often between the true glass films, beautiful circular crystals of silex. In one, around a minute speck of silex, there is found a circular band of equally minute crystalline specks, and at a greater distance a second circular band, concentric with the first, consisting of still smaller silicious particles hardly visible in the microscope. And he asks, by what atomic forces does this central crystal group its attendant crystals around it?

Sir David's paper was followed by one *on the Motion of a Pendulum in a Vertical Plane when the Point of Suspension moves uniformly on a Circumferenee in the same Plane*, by Professor PIERCE. The author wrote down the mathematical formulæ which gave the laws which govern such motions. He then exhibited beautifully executed diagrams on transparent cloth, which showed by curves, some most regular and some most fantastic in their forms, the behaviour of such a pendulum under various conditions, and at several periods of its course. He pointed out cases in which these curves exhibited all the symmetry and regularity of exact mathematical forms, others in which these forms were complicated and irregular almost beyond conception. He showed that in some of these cases the state of the pendulum was that of a stable equilibrium, whilst in others the equilibrium was unstable, and the pendulum went off into the most rapid motions. By another series of curves, something like Contour's lines, he showed how the succession of these motions could all be tracked; and he concluded by showing how a similar method was applicable to the tracing of matter through its several varieties of forms,—inorganic matter being analogous to the changes and varieties observed in the state of stable equilibrium, while the various states of unstable equilibrium gave many of the surprising and irregular transitions observed in the vegetable and animal kingdoms, or in organised matters.

On the last day of the meeting of the Section, it received a proposal for an atmotic ship by the Hon. W. Bland, N. S. Wales, by which he proposed to navigate the air as he pleased by means of heavy weights, light vanes like those of a screw propeller, and a balloon. In the sub-Section, the communication of greatest interest was a letter from Captain Maury, U. S. Navy, on Antarctic climates and expeditions.

SECTION B.—CHEMICAL SCIENCE.

In this Section, the papers read were mostly of a technical nature, not easily admitting of an abstract. Among others of interest we may mention an experiment of W. R. Grove, which proved that electrolysis by a Ruhmkorff's coil in water acidulated with sulphuric acid may go on though the glass of a Florence flask be interposed between the electrodes on all sides. Dr Lyon Playfair also resumed his theory of neutral salts as represented not on the type of a basic oxide $H_2 O_2$, but a neutral peroxide HO_2 and constructed according to the laws of symmetry. The same day, Dr Gladstone showed that when equivalent proportions of chloride of sodium and nitrate of baryta are mixed together in solution, and diffused, four salts exist contemporaneously in the liquid; or in other words, a portion of each acid combines with a portion of each base. Dr Andrews resumed his observations on ozone. J. B. Lawes and Dr J. H. Gilbert continued their researches in the philosophy of agriculture, contributing this year a paper on the composition of the ash of wheat grown under various circumstances. Besides these, papers were read, by J. J. Coleman, on some remarkable relations existing between the atomic weights, atomic volumes, and properties of chemical elements; on the deodorization of sewerage, by Dr Bird; on a new organic compound containing boron, by Dr Frankland and Mr Duppa; on the occurrence of poisonous metals in cheese, by Professor Voelcker, in consequence, as he ascertained, in reference to the dairies of Gloucester and Wiltshire, of the use of the sulphates of copper and of zinc in the manufacture.

SECTION C.—GEOLOGY.

This section was opened by an address by Professor SEDGWICK, its President. The remarks we have made as to the Chemical Section, however, apply also to this; and with a single exception, of which we shall give an extract, we shall merely enumerate the papers brought forward. The paper of which we give an abstract was on the contents of three square yards of triassic drift near Frome, by C. Moore of Bath. In order that they might receive a more careful examination than could be given to it on the spot, the whole of it, consisting of about three tons weight, was carted away to the residence of the author at Bath, a distance of twenty miles; all of which had passed under his observation, with the following results:—The fish remains, which were the most abundant, were first noticed. Some idea might be formed of their numbers when he stated that of the genus Acrodus alone, including two species, he had extracted 45,000 teeth from the three square yards of earth under notice, and that they were even more numerous than these numbers indicated, since he rejected all but the most perfect examples. Teeth of the Saurichthys of several species were also abundant; and, next to them, teeth of the Hybodus, with occasional spines of the latter genus. Scales of Gyrolepis and Lepidotus were also numerous, and teeth showing the presence of several other genera of fishes. With the above were found a number of curious bodies, each of which was surmounted by a depressed, enamelled, thorn-like spine or tooth, in some cases with points as sharp as that of a coarse needle; these the author supposed to be spinous scales, belonging to several new species of fish, allied to the Squaloraia, and that to the same genus were to be referred a number of hair-like spines, with flattened fluted sides, found in the same deposit. There were also present specimens, hitherto supposed to be teeth, and for which Agassiz had created the genus Ctenoptychius, but which he was rather disposed to consider—like those previously referred to—to be the outer scales of a fish allied to the Squaloraia. It was remarked that, as the drift must have been transported from some distance, delicate organisms could scarcely have been expected; but, notwithstanding, it contained some most minute fish-jaws and palates, of which the author had, either perfect or otherwise, 130 examples. These were from a quarter to the eighth of an inch in length, and within this small compass he possessed specimens with from thirty to forty teeth; and in one palate he had succeeded in reckoning as many as seventy-four teeth in position, and there were spaces where sixteen more had disappeared, so that, in this tiny specimen, there were ninety teeth! Of the order Reptilia there were probably eight or nine genera, consisting of detached teeth, scutes, vertebræ, and ribs, and articulated bones. Amongst these he had found the flat crushing teeth of the Placodus: a discovery of interest, for hitherto this reptile had only been found in the muschelkalk of Germany,—a zone of rocks hitherto wanting in this country, but which, in its Fauna, was represented by the above reptile. But by far the most important remains in the deposit were indications of the existence of triassic mammalia. Two little teeth of the Microlestes had some years before been found in Germany, and were the only traces of this high order in beds older than the Stonesfield slate. The author's minute researches had brought to light fifteen molar teeth, either identical with or allied to the Microlestes, and also five incisor teeth, evidently belonging to more than one species. A very small double-fanged tooth, not unlike the oolitic Spalacotherium, proved the presence of another genus; and a fragment of a tooth, consisting of a single fang, with a small portion of the crown attached, a third genus, larger in size than the Microlestes. Three vertebræ, belonging to an animal smaller than any existing mammal, had also been found. The author inferred that, if twenty-five teeth and vertebræ, belonging to three or four genera of Mammalia, were to be found within the space occupied by three square yards of earth, that portion of the globe which was then dry land, and from whence the material was in part derived, was probably inhabited at this early period of its history by many genera of Mammalia, and would serve to encourage a hope that this family might yet be found in beds of even a more remote age. A discussion followed, in which Sir C. Lyell, Professor Sedgwick, Dr H. Falconer, and others took part, when the importance of the author's discoveries was recognised.

The other papers were—On the Osseous Caves of Tenby, by the Rev. G. N. Smith. Sir R. I. Murchison exhibited the New Geological Map of Oxford. On Snow Crystals observed at Dresden, by Dr Geinitz; On the Silurian Formation in the District of Wilsdruff, by Dr Geinitz; On the Metamorphic Rocks of the North of Ireland, by Professor Harkness; On the Intermittent Springs of the Chalk and Oolite of the Neighbourhood of Scarborough, by Captain Woodall; Report on the Dura Den Excavations, by Dr Anderson; On Circular Chains in the Alps, by M. A. Favre; On a Recent Volcanic Eruption in Iceland, by Dr W. S. Lindsay; Details respecting a Nail found in Kingoodie Quarry, by Sir D. Brewster; On the Tynedale Coalfield and Whinsil, by J. A. Knipe; On Slikensides, by J. Price; Notes on the Geology of Captain Palliser's Route across the Rocky Mountains, by Dr Hector; On the Geology of the Vicinity of Oxford, by Professor Phillips; On the Invertebrate Fauna of the Lower Oolites of Oxfordshire, by J. F. Whiteaves; On the Blenheim Iron Ore, and the Thickness of the Formations below the Great Oolite at Stonesfield, by E. Hull; On the Stratigraphical Position of certain Species of Coral in the Lias, by the Rev. P. B. Brodie; On the Geological Characters of the Sahara, by the Rev. H. B. Tristram; On the Mode of Flight of the Pterodactyles of the Coprolite Bed near Cambridge, by the Rev. J. B. P. Dennis; Remarks on the Elevation Theory of Volcanoes, by Dr Daubeny; Notes on some Points in Chemical Geology, by T. Sterry Hunt; On the Geographical and Chronological Distribution of Devonian Fossils in Devon and Cornwall, by W. Pengelly; On the *Avicula Contorta* Bed, and Lower Lias in the South of England, by Dr Wright; On some New Facts in Relation to the Section of the Cliff at Mundsley, Norfolk, by Joseph Prestwich; On the Igneous Rocks interstratified with the Carboniferous Limestone of the Basin of Limerick, by Professor Jukes; On the Stratigraphical Position of certain Species of Corals in the Lias, by the Rev. P. B. Brodie; On some Reptilian Footprints from the New Red Sandstone North of Wolverhampton, by the Rev. W. Lister; On the Effects of long-continued Heat, shown in the Iron Furnaces of the West of Yorkshire, by the Rev. W. V. Harcourt; On some Phenomena of Metamorphism in Coal in the United States, by Professor Rogers; On the Geology of the Vicinity of the Neighbourhood of Cambridge, and the Fossils of the Upper Green Sand, by Professor Sedgwick; Some Observations upon the Geological Features of the Volcanic Island of St Paul, in the South Indian Ocean, illustrated by a Model in Relief of the Island, made by Captain Cybulz, of the Austrian Artillery, by Professor F. von Hochstetter; Remarks on the Geology of New Zealand, illustrated by Geological Maps, Drawings, and Photographs, by Professor F. von Hochstetter; On some Transformations of Iron Pyrites in connection with Fossil Remains, by A. Gages; Remarks on Fossil Fish from the North Staffordshire Coal-Fields, by W. Molyneux; On the Old Red Sandstone and its Fossil Fish in Forfarshire, with an Account of the Fish by Sir P. Egerton communicated by Sir R. I. Murchison, by W. Powrie; On a New Form of Ichthyolite discovered by Mr Peach, by Sir P. Egerton; On Two Newly-discovered Caves in Sicily containing Worked Flints, by Baron F. Anca; On the Six-inch Maps of the Geological Survey, by E. Hull; On the Selection of a Peculiar Geological Habitat by some of the rarer British Plants, by the Rev. W. Symonds; On the Koh-i-Noor previous to its Cutting, by the Rev. W. Mitchell and Professor Tennant.

SECTION D.—ZOOLOGY AND BOTANY, INCLUDING PHYSIOLOGY.

(A sub-Section attached for the last-named subject.)

It was to this Section that the chief interest attached, in consequence of the popularity at the present moment of discussion as to the origin of species. After a Report by Dr Ogilvie, intimating the little that had been done, in consequence of the tempestuous weather and the early meeting of the Association, by the Dredging Committee for the North and East Coasts of Scotland, and a very interesting communication by the Rev. P. P. Carpenter, on the Progress of Natural Science in the United States and Canada,—Dr DAUBENY led off in the great question of the day, by a paper on the Final Causes of the Sexuality of Plants, with particular reference to Mr. Darwin's work on the Origin of Species by Natural Selection.

Dr Daubeny began by pointing out the identity between the two modes by which the multiplication of plants is brought about, the very same properties being imparted to the bud or to the graft as to the seed produced by the ordinary process of fecundation, and a new individual being in either instance equally produced. We are therefore led to speculate as to the final cause of the existence of sexual organs in plants, as well as in those lower animals which can be propagated by cuttings. One use, no doubt, may be the dissemination of the species; for many plants, if propagated by buds alone, would be in a manner confined to a single spot. Another secondary use is the production of fruits which afford nourishment to animals. A third may be to minister to the gratification of the senses of man by the beauty of their forms and colours. But as these ends are only answered in a small proportion of cases, we must seek further for the uses of the organs in question; and hence the author suggested that they might have been provided in order to prevent that uniformity in the aspect of Nature which would have prevailed if plants had been multiplied exclusively by buds. It is well known that a bud is a mere counterpart of the stock from whence it springs, so that we are always sure of obtaining the very same description of fruit by merely grafting a bud or cutting of a pear or apple tree upon another plant of the same species. On the other hand, the seed never produces an individual exactly like the plant from which it sprang; and hence, by the union of the sexes in plants, some variation from the primitive type is sure to result. Dr Daubeny remarked that if we adopt in any degree the views of Mr Darwin with respect to the origin of species by natural selection, the creation of sexual organs in plants might be regarded as intended to promote this specific object. Whilst, however, he gave his assent to the Darwinian hypothesis, as likely to aid us in reducing the number of existing species, he wished not to be considered as advocating it to the extent to which the author seems disposed to carry it. He rather desired to recommend to naturalists the necessity of farther inquiries, in order to fix the limits within which the doctrine proposed by Mr Darwin may assist us in distinguishing varieties from species.

Professor Huxley having been called on by the chairman, deprecated any discussion on the general question of the truth of Mr Darwin's theory. He felt that a general audience, in which sentiment would unduly interfere with intellect, was not the public before which such a discussion should be carried on. Dr Daubeny had brought forth nothing new to demand or require remark.—Mr R. Dowden, of Cork, mentioned, first, two instances in which plants had been disseminated by seeds, which could not be affected by buds, first, in the introduction of *Senicio squalida*, by the late Rev. W. Hincks; and, second, in the diffusion of chicory, in the vicinity of Cork, by the agency of its winged seeds. He related several anecdotes of a monkey, to show that however highly organised the Quadrumana might be, they were very inferior in intellectual qualities to the dog, the elephant, and other animals. He particularly referred to his monkey being fond of playing with a hammer; but although he liked oysters as food, he never could teach him to break the oysters with his hammer as a means of indulging his appetite.—Dr Wright stated that a friend of his, who had gone out to report on the habits of the gorilla—the highest form of monkey—had observed that the female gorilla took its young to the sea-shore for the purpose of feeding them on oysters, which they broke with great facility.

Professor Owen said that he wished to approach this subject in the spirit of the philosopher, and expressed his conviction that there were facts by which the public could come to some conclusion with regard to the probabilities of the truth of Mr Darwin's theory. Whilst giving all praise to Mr Darwin for the courage with which he had put forth his theory, he felt it must be tested by facts. As a contribution to the facts, by which the theory must be tested, he would refer to the structure of the highest Quadrumana as compared with man. Taking the brain of the gorilla, it presented more differences, as compared with the brain of man, than it did when compared with the brains of the very lowest and most problematical form of the Quadrumana. The differences in cerebral structure between the gorilla and man were immense. The posterior lobes of the cerebrum in man presented parts which were wholly absent in the gorilla. The same remarkable differences of structure were seen in other parts of the body; yet he would especially refer to the structure of the great toe in man, which was constructed to enable him to assume the upright position; whilst in the lower monkeys it was impossible, from the structure of their feet, that they should do so. He concluded by urging on the physiologist the necessity of experiment. The chemist, when in doubt, decided his questions by experiment; and this was what is needed by the physiologist.—Professor Huxley begged to be permitted to reply to Professor Owen. He denied altogether that the difference between the brain of the gorilla and man was so great as represented by Professor Owen, and appealed to the published dissections of Tiedemann and others. From the study of the structure of the brain of the Quadrumana, he maintained that the difference between man and the highest monkey was not so great as between the highest and the lowest monkey. He maintained also, with regard to the limbs, that there was more difference between the toeless monkeys and the gorilla than between the latter and man. He believed that the great feature which distinguished man from the monkey was the gift of speech.

This subject was resumed another day by a paper *on the Intellectual Development of Europe, considered with Reference to the Views of Mr Darwin and others, that the Progression of Organisms is determined by Law*, by Professor Draper, M.D., of New York. The object of this paper was to show that the advancement of man in civilisation does not occur accidentally or in a fortuitous manner, but is determined by immutable law. The author introduced his subject by recalling proofs of the dominion of law in the three great lines of the manifestation of life. First, in the successive stages of development of every individual, from the earliest rudiment to maturity; secondly, in the numberless organic forms now living contemporaneously with us, and constituting the animal series; thirdly, in the orderly appearance of that grand succession which in the slow lapse of geological time has emerged, constituting the life of the Earth, showing therefrom not only the evidences, but also proofs of the dominion of law over the world of life. In those three lines of life he established that the general principle is, to differentiate instinct from automatism, and then to differentiate intelligence from instinct. In man himself three distinct instrumental nervous mechanisms exist, and three distinct modes of life are perceptible, the automatic, the instinctive, the intelligent. They occur in an epochal order, from infancy through childhood to the more perfect state. Such holding good for the individual, it was then affirmed that it is physiologically impossible to separate the individual from the race, and that what holds good for the one holds good for the other too; and hence that man is the archetype of society, and individual development the model of social progress, and that both are under the control of immutable law: that a parallel exists between individual and national life in this, that the production, life, and death of an organic particle in the person, answers to the production, life, and death of a person in the nation. Turning from these purely physiological considerations to historical proof, and selecting the only European nation which thus far has offered a complete and completed intellectual life, Professor Draper showed that the characteristics of Greek mental development answer perfectly to those of individual life, presenting philosophically five well marked ages or periods,—the first being closed by the opening of Egypt to the Ionians; the second, including the Ionian, Pythagorean, and Eleatic philosophies, was ended by the criticisms of the Sophists; the third, embracing the Socratic and Platonic philosophies, was ended by the doubts of the Sceptics; the fourth, ushered in by the Macedonian expedition, and adorned by the splendid achievements of the Alexandrian school, degenerated into Neoplatonism and imbecility in the fifth, to which the hand of Rome put an end. From the solutions of the four great problems of Greek philosophy, given in each of these five stages of its life, he showed that is possible to determine the law of the variation of Greek opinion, and to establish its analogy with that of the variations of opinion in individual life. Next, passing to the consideration of Europe in the aggregate, Professor Draper showed that it has already in part repeated these phases in its intellectual life. Its first period closes with the spread of the power of Republican Rome, the second with the foundation of Constantinople, the third with the Turkish invasion of Europe; we are living in the fourth. Detailed proofs of the correspondence of these periods to those of Greek life, and through them to those of individual life, are given in a work now printing on this subject, by the author, in America. Having established this conclusion, Professor Draper next briefly alluded to many collateral problems or inquiries. He showed that the advances of men are due to external and not to interior influences, and that in this respect a nation is like a seed, which can only develop when the conditions are favourable, and then only in a definite way; that the time for psychical change corresponds with that for physical, and that a nation cannot advance except its material condition be touched,—this having been the case throughout all Europe, as is manifested by the diminution of the blue-eyed races thereof; that all organisms, and even man, are dependent for their characteristics, continuance and life, on the physical conditions under which they live; that the existing apparent invariability presented by the world of organisation is the direct consequence of the physical equilibrium; but that if that should suffer modification, in an instant the fanciful doctrine of the immutability of species would be brought to its proper value. The organic world appears to be in repose because natural influences have reached an equilibrium. A marble may remain motionless for ever on a level table, but let the table be a little inclined, and the marble will quickly run off; and so it is with organisms in the world. From his work on Physiology, published in 1856, he gave his views in support of the doctrine of the transmutation of species; the transitional forms of the animal to the human type; the production of new ethnical elements, or nations; and the laws of their origin, duration, and death.

The announcement of this paper attracted an immense audience to the Section, which met in the Library of the New Museum. The discussion was commenced by the Rev. Mr Cresswell, who denied that any parallel could be drawn between the intellectual progress of man and the physical development of the lower animals. So far from Professor Draper being correct with regard to the history of Greece, its masterpieces in literature—the Iliad and Odyssey—were produced during its national infancy. The theory of intellectual development proposed was directly opposed to the known facts of the history of man.—Sir B. Brodie stated he could not subscribe to the hypothesis of Mr Darwin. His primordial germ had not been demonstrated to have existed. Man had a power of self-consciousness—a principle differing from anything found in the material world—and he did not see how this could originate in lower organisms. This power of man was identical with the Divine Intelligence; and to suppose that this could originate with matter, involved the absurdity of supposing the source of Divine power dependent on the arrangement of matter.—The Bishop of Oxford stated that the Darwinian theory, when tried by the principles of inductive science, broke down. The facts brought forward did not warrant the theory. The permanence of specific forms was a fact confirmed by all observation. The remains of animals, plants, and man found in those earliest records of the human race the Egyptian catacombs, all spoke of their identity with existing forms, and of the irresistible tendency of organised beings to assume an unalterable character. The line between man and the lower animals was distinct: there was no tendency on the part of the lower animals to become the self-conscious intelligent being Man; or in man to degenerate and lose the high characteristics of his mind and intelligence. All experiments had failed to show any tendency in one animal to assume the form of the other. In the great case of the pigeons quoted by Mr Darwin, he admitted that no sooner were these animals set free than they returned to their primitive type. Everywhere sterility attended hybridism, as was seen in the closely allied forms of the horse and the ass. Mr Darwin's conclusions were an hypothesis, raised most unphilosophically to the dignity of a causal theory. He was glad to know that the greatest names in science were opposed to this theory, which he believed to be opposed to the interests of science and humanity.—Professor Huxley defended Mr Darwin's theory from the charge of its being merely an hypothesis. He said it was an explanation of phenomena in Natural History, as the undulating theory was of the phenomena of light. No one objected to that theory because an undulation of light had never been arrested and measured. Darwin's theory was an explanation of facts;

and his book was full of new facts, all bearing on his theory. Without asserting that every part of the theory had been confirmed, he maintained that it was the best explanation of the origin of species which had yet been offered. With regard to the psychological distinction between man and animals, man himself was once a monad—a mere atom; and nobody could say at what moment in the history of his development he became consciously intelligent. The question was not so much one of a transmutation or transition of species, as of the production of forms which became permanent. Thus the short-legged sheep of America were not produced gradually, but originated in the birth of an original parent of the whole stock, which had been kept up by a rigid system of artificial selection.—Admiral Fitzroy regretted the publication of Mr Darwin's book, and denied Professor Huxley's statement, that it was a logical arrangement of facts.—Dr Beale pointed out some of the difficulties with which the Darwinian theory had to deal, more especially those vital tendencies of allied species which seemed independent of all external agents.—Mr Lubbock expressed his willingness to accept the Darwinian hypothesis in the absence of any better. He would, however, express his conviction, that time was not an essential element in these changes. Time alone produced no change.—Dr Hooker being called upon by the President to state his views of the botanical aspect of the question, observed that the Bishop of Oxford having asserted that all men of science were hostile to Mr Darwin's hypothesis, whereas he himself was favourable to it, he could not presume to address the audience as a scientific authority. As, however, he had been asked for his opinion, he would briefly give it. In the first place, his Lordship, in his eloquent address, had, as it appeared to him, completely misunderstood Mr Darwin's hypothesis. His Lordship intimated that this maintained the doctrine of the transmutation of existing species one into another, and had confounded this with that of the successive development of species by variation and natural selection. The first of these doctrines was so wholly opposed to the facts, reasonings, and results of Mr Darwin's work, that he could not conceive how any one who had read it could make such a mistake—the whole book, indeed, being a protest against that doctrine. Then, again, with regard to the general phenomena of species, he understood his Lordship to affirm that these did not present characters that should lead careful and philosophical naturalists to favour Mr Darwin's views. To this assertion Dr Hooker's experience of the vegetable kingdom was diametrically opposed. He considered that at least one-half of the known kinds of plants were disposable in groups, of which the species were connected by varying characters common to all in that group, and sensibly differing in some individuals only of each species; so much so, that if each group be likened to a cobweb, and one species be supposed to stand in the centre of that web, its varying characters might be compared to the radiating and concentric threads, when the other species would be represented by the points of union of these; in short, that the general characteristics of orders, genera, and species amongst plants differed in degrees only from those of varieties, and afforded the strongest countenance to Mr Darwin's hypothesis. As regarded his own acceptation of Mr Darwin's views, he expressly disavowed having adopted them as a creed. He knew no creeds in scientific matters. He had early begun the study of natural science under the idea that species were original creations; and it should be steadily kept in view that this was merely another hypothesis, which in the abstract was neither more nor less entitled to acceptance than Mr Darwin's; neither was, in the present state of science, capable of demonstration, and each must be tested by its power of explaining the mutual dependence of the phenomena of life. For many years he had held to the old hypothesis, having no better established one to adopt, though the progress of botany had in the interim developed no new facts that favoured it, but a host of most suggestive objections to it. On the other hand, having fifteen years ago been privately made acquainted with Mr Darwin's views, he had during that period applied these to botanical investigations of all kinds in the most distant parts of the globe, as well as to the study of some of the largest and most different Floras at home. Now, then, that Mr Darwin had published it, he had no hesitation in publicly adopting his hypothesis, as that which offers by far the most probable explanation of all the phenomena presented by the classification, distribution, structure, and development of plants in a state of nature and under cultivation, and he should therefore continue to use his hypothesis as the best weapon for future research, holding himself ready to lay it down should a better be forthcoming, or should the now abandoned doctrine of original creations regain all it had lost in his experience.

Dr Lankester read a paper for Mr Hogg, *on a Fourth Kingdom of Nature.* The author stated the great difficulty he had long experienced when examining some of the simpler living beings, in defining the characters of those primary forms of life, whether they belong to the vegetable or animal kingdom. And since it appears to many desirable to place those organic beings which are of a doubtful nature in a fourth or an additional kingdom, he suggested one under the title of the Primigenal Kingdom—Regnum Primigenum continens Protoctista, *i. e.*, Protophyta et Protozoa. This would comprise all the lower creatures, or the primary organic beings, "Protoctista," from *πρῶτος*, *first*, and *κτιστὰ*, *created beings*, both Protophyta and Protozoa, and would also include the Sponges or Amorphozoa of M. de Blainville, although Mr J. Hogg thought it better to substitute for the former the name of Amorphoctista; derived from *ἄμορφος*, *formless*, and *κτιστὰ*, *creatures*, or organisms. Some having compared the vegetable and animal kingdoms to *two* pyramids, which diverge from each other as they ascend, but are placed on a common base, the author conceived that that *base* might fairly represent the primigenal kingdom, which embraces the lower or primary organisms of both the former, but which are of a doubtful nature, and can in some instances only be considered as having become blended or mingled together.

An accompanying diagram was exhibited, which represented the two pyramids springing from the same base; one, coloured yellow, denoted the vegetable kingdom, the other was tinged blue, and signified the animal kingdom; whilst the base, common to both, was coloured green, which was intended to show by the *union* of the two former colours the blending of the two natures of the lower created beings comprised in the fourth or primigenal kingdom. These pyramids, with their base, stood on a foundation tinged brown, thereby signifying the earth and the mineral kingdom. (This paper appears in the present number of this Journal, page 216.)

Dr Lankester could not agree with the author as to the necessity of a fourth kingdom in nature.

Dr COLLINGWOOD read also a paper *on Recurrent Animal Form, and its Significance in Systematic Zoology.* The object of this paper was to call attention to the frequent recurrence of similar forms in widely-separated groups of the animal kingdom, similarities, therefore, which were unaccompanied by homologies of internal structure. These analogies of form had greatly influenced the progress of classification, by attracting the attention of systematizers while as yet structural homologies were imperfectly understood, and, as a consequence, many groups of animals had been temporarily located in a false position, such as bats and whales by the ancients, and the Polyzoa and Foraminifera in more modern times. These resemblances in form were illustrated generally by the classes of Vertebrata, and more especially by the various orders of Mammalia—the Invertebrata, affording, however, many remarkable examples. Since no principle of gradation of form would sufficiently account for these analogies, the author had endeavoured to discover some other explanation, and had come to the conclusion, that the fact of deviations from typical *form* being accompanied by modifications of typical *habits*, afforded the desired clue. Examples of this were given, and the principle educed, that *agreement of habit and economy in widely-separated groups is accompanied by similarity of form.* This position was argued through simple cases to the more complex, and the conclusion arrived at, that where habits were *known*, the explanation sufficed; and it was only in the case of animals of low organisation and obscure or unknown habits, that any serious difficulty arose in its application, so that our appreciation of the *rationale* of their similarity of form was in direct ratio to our knowledge of their habits and modes of life. In conclusion, by a comparison of the Polyzoa with the Polyps, it was shown that the economy of both was nearly identical, although they possessed scarcely anything in common except superficial characters, and this identity of habit was regarded as the explanation of their remarkable similarity of form.

Besides these there were many other papers of much interest before the Section, but the most elaborate was that on the British Teredines or ship-worms, by Mr Jeffreys. After observing that his researches had not been confined to the British Teredines, but that he had recently had an opportunity of meeting all the French naturalists who had published on the subject, as well as of studying all the accessible collections and books. He treated the matter first in a zoological point of view, and gave a short history of the genus Teredo from the time of Aristotle and his pupil Theophrastus to the present time; especially noticing the elaborate monograph of Sellius, in 1733, on the Dutch ship-worm; the valuable paper of Sir Everard Home and his pupil Sir Benjamin Brodie, in 1806; and the physiological essays of Quatrefages, in 1849. He showed that the Teredo undergoes a series of metamorphoses; the eggs being developed into a sub-larval form after their exclusion from the ovary, and remaining in the mouth of the parent for some time. In its second phase (or that of proper larvæ), the fry are furnished with a pair of close-fitting oval valves, resembling those of a Cythere, as well as with cilia, a large foot, and distinct eyes, by means of which it swims freely and with great rapidity, or creeps, and afterwards selects its fixed habitation. The larval state continues for upwards of 100 hours, and during that period the fry are capable of traversing long distances, and thus becoming spread over comparatively wide areas. The metamorphosis is not, however as (Quatrefages asserts), complete; because the young shell, when fully developed, retains the larval valves. He then discussed the different théories, as to the method by which the Teredo perforates wood, giving a preference to that of Sellius and Quatrefages, which may be termed the theory of "suction," aided by a constant maceration of the wood by water, which is introduced into the tube by the syphons. This process, according to Quatrefages, is effected by an organ which he calls the "*capuchon céphalique*," and which is provided with two pairs of muscles of extraordinary strength. Mr Jeffreys instanced, in illustration of his theory, the cases of the common limpet, as well as of many bivalve molluscs, *Echinus lividus*, and numerous annelids, which excavate rocks to a greater or less depth; and he cited the adage of "Gutta cavat lapidem non vi sed sæpe cadendo," in opposition to the mechanical theory. The Teredo bores either in the direction of the grain or across it, according to the kind of wood and the nature of the species; the *Teredo Norvagica* usually taking the former course: every kind of wood is indiscriminately attacked by it. The Teredines constitute a peaceful though not a social community; and they have never been known to work into the tunnel of any neighbour. If they approach too near to each other, and cannot find space enough in any direction to continue their operations, they inclose the valves or anterior part of the body in a case consisting of one or more hemispherical layers of shelly matter. Sellius supposed that the Teredo ate up the wood which it excavated, and had no other food; and, labouring under the idea that it could no longer subsist after being thus voluntarily shut up, he considered it to be the pink of chivalry and honour, in preferring to commit suicide rather than infringe on its neighbour. In this inclosed state the valves often become so much altered in form, as well as in the relative proportion of their different parts, as not to be easily recognisable as belonging to the same species; and one species (*T. divaricata*) was constituted from specimens of *T. Norvagica* which had been so deformed. The food of the Teredo consists of minute animalculæ, which are brought within the vortex of the inhalant syphon, and drawn into the stomach. The wood which has been excavated also undergoes a kind of digestion during its passage outwards through the long intestine. The animal has been proved by Laurent and other observers to be capable of renewing its shelly tube, and of repairing it in any part. It is stated by Quatrefages (and apparently with truth) that the sexes are separate, impregnation being effected in a similar mode to that which takes place among palm-trees and other diœcious plants. There appear to be only five or six males in one hundred individuals. The Teredo perforates and inhabits sound wood only, but an allied genus (Xylophaga) has been recently found to attack the submarine telegraph cable between this country and Gibraltar at a depth of from sixty to seventy fathoms, and to have made its way through a thick wrapper of cordage into the gutta percha which covered the wire. The penetration was fortunately discovered in time, and was not deep enough to reach the wire. He gave several instances to show the rapidity of its perforating powers,—one of them having been supplied by Sir Leopold M'Clintock while he was serving with the author's brother in the North Pacific. Mr Jeffreys then traced the geographical distribution of the Teredines, and showed that at least two species, which are now found living on our own shores, occurred in the post-pleistocene period; and he inferred from the circumstance of one of these species having been found in fossil *drift* wood, that conditions similar to the present existed during that epoch. Some species inhabit fixed wood, and may be termed "littoral," while others are only found in floating wood, and appear to be "pelagic." Each geographical district has its own "littoral" species; and the old notion of the shipworm (which Linnæus justly called "*Calamitas Navium*") having been introduced into Europe from the Indies was contrary to fact as well as theory, because no "littoral" species belonging to tropical seas has ever been found living in the northern hemisphere, or *vice versâ*. It is true that some species have been occasionally imported into this and other countries in ships' bottoms, and that others occur in wood which has been wafted thither by the Gulf and other oceanic currents; but the fewer cases belong to littoral species, and never survive their removal, while the latter may be said to be almost cosmopolite. Every species of Teredo has its own peculiar tube, valves, and pair of "pallets," the latter serving the office of opercula, and by their means the animal is able at will to completely close the entrance or mouth of the tube, and thus prevent the intrusion of crustacean and annelidan foes. The length of the tube is of course equal to that of the animal, which is attached to it by strong muscles in the palletal-ring, and varies in the different species from three inches, or even less, to as many feet. The internal entrance or throat of the tube is also distinguishable in each species by its peculiar transverse laminæ, and frequently a longitudinal siphonal ridge. Monstrosities not unfrequently occur in the valves and pallets; and in one instance the pallet-stalk is double, showing a partial redundancy of organs, as exemplified by the author with respect to the operculum of the common whelk. More than one species often inhabit the same piece of wood; and want of sufficient care by naturalists in extracting the valves with their proper tubes and pallets may account in a great measure for the confusion which exists in public and private collections, and which has thence found its way into systematic works. The Teredines have many natural enemies, both in life and after death. In the south of Italy, and on the North African coast, they are esteemed as human food. In Great Britain and Ireland, four species occur in fixed wood, and eleven others in drift wood, the latter being occasional visitants. Of these, no less than six have never yet been described, and two others are now, for the first time noticed as British. The number of recorded exotic species only amounts to six more, making a total of twenty-one; but it is probable that when the subject has been more investigated, a considerable addition will be made to this number. Mr Jeffreys then explained the distribution of the littoral species on the shores of Great Britain and Ireland, and produced a synoptical list with descriptions of the new species. He believed all the Teredines were marine, except possibly Adanson's Senegal species, and one which had lately been found in the river Ganges, the water of which is fresh for about eighteen hours out of the twenty-four, and brackish during the rest of the day; but as a well-known exception of the same kind occurs in a genus of marine shells (Arca), and the transition from fresh to brackish, and thence to salt water, is very gradual, such exceptions should not be regarded with suspicion or surprise. He concluded this part of the subject by exhibiting some drawings and specimens, and acknowledging his obligations to Dr Lukis and other scientific friends. He next treated the subject in an economical point of view, and remarked, that although the French government had issued two commissions at different times, and the Dutch government has lately published the report of another commission, which was appointed to inquire into the mode of preventing the ravages of the Teredo in the ships and harbours of those countries, our own government had done nothing. He alluded to the numerous and various remedies which had been proposed during the last two or three centuries, from time to time, some of which were very absurd; but he considered, from a study of the creature's habits, that the most effectual preventive would be a silicious or mineral composition, like that which has been proposed by Professor Ansted for coating the decomposing stones of our new Houses of Parliament, or simply a thick coat of tar or paint, continually applied, which would not only destroy any adult ship-worms then living in the wood, but prevent the ingress of the fry. The Teredo never commences perforation except in the larval state.

A committee of the Association has been formed, at the suggestion of Mr Jeffreys, to inquire and report as to the best mode of preventing the ravages of Teredo and other animals in our ships and harbours.

Professor Van der Hoeven referred to the fact, that the ship-worm attacked ships more one season than another. In 1858, they committed great ravages on the ships of Holland, and a committee of the Dutch Academy of Sciences was appointed to investigate the subject. Professor Verloren stated that the species which attacked the ships of Holland was *Teredo navalis;* but the species in Norway, France, and England, were sometimes different. Sir W. Jardine expressed his surprise that the government had not appointed a committee to investigate the subject. Professor Huxley stated that probably the House of Commons had had too much experience of the utter inutility of attempting to stop a bore, to undertake the subject. Dr E. P. Wright exhibited some specimens of a new genus of Teredine, which he called *Halidaia.* It occurred near Feruckpore, in India, and inhabited perfectly fresh water. It was one of the largest species known, and the first which had been found in fresh water.

Papers read at the American Association for the Advancement of Science, at the Meeting held at Newport, Aug. 1, 1860.

Professor Elias Loomis *on the great Auroral Display of August* 28 *and September* 2, 1859. These displays, he said, are probably unsurpassed, and a greater amount of information has been collected about them than was ever before collected. This information furnishes materials for settling several important points. The first display was seen over about two-thirds of the globe, the second over the whole globe. Both conform to the general law, that the region of the greatest polar action is about 15° further south in the United States than in Western Europe. By a comparison of observations, it appears that the aurora of August 28 extended through a space from 530 to 40 miles above the earth's surface, and that of September 2 from 490 to 50 miles above. The illumination consisted chiefly of illumined paths parallel to the axis of the needle. The telegraph, and various tests and experiments in connection with it, show that during the phenomena, electric currents were developed equal to the ordinary full strength of a Voltaic battery, or, in technical terms, to 200 cups of Grove's battery. This electricity must have been derived from the aurora, either by transfer or induction; if by transfer, the electricity is of the same character. He was compelled to admit that the auroral current is electricity; its colour is just the same as that of electricity passing through rarified air. The aurora has a tendency to periodicity, or rather a displacement of the auroral region.

Professor C. H. HITCHCOCK of Amherst explained the synchronism of the coal beds in the Rhode Island and Western United States Coal Basins, arguing, from their fossil remains, that they form a connecting link between the Appalachian and Nova Scotian coals and those of the West.

Professor J. S. Newberry argued that the fossil remains did not wholly show the synchronism of the coal measures. Professor Wm. B. Rogers said that in our early attempts to trace the continuity of single coal seams we are often led astray. Coal measures may contain the same fossils, and yet not have been deposited at the same time. Professor Agassiz took the same view, and said that it was probable that our peat bogs of the north and cedar swamps of the south may at some and the same time become coal beds, and yet their fossils would differ. So deposits formed at the same time, and not far distant, may not contain a single identical fossil, and our old method may therefore lead us to error. Again, the deposits of a very long period may be of very small thickness. Thus the coral beds of Florida, although but sixty or seventy feet thick, were probably begun before man was created; and it may turn out that the carboniferous epoch is really more than one, perhaps even ten cosmic periods. He was satisfied that there is no better way of identifying rocks than by the study of fossils, but the study of the geographical distribution of animals on the present surface of the earth should precede the attempt at classifying periods or strata by their fossils.

A communication by Dr C. JOHNSON of Baltimore was read, upon a diatomaceous earth from Nottingham, Calvert Co., Md., arguing that it is the same as Bermuda tripoli.

Professor W. A. NORTON read an abstract of a *Memoir on the Theoretical Determination of Donati's Comet.* It elicited considerable discussion. He argued that the luminous train of the comet was composed of two descriptions of cometary light, made up of particles variously repelled and attracted by the sun, and with the variation of the repulsion and attraction the expansion varies.

Mr B. F. **Harrison** read a paper on the *Solution of Ice in Inland Waters*, accounting for the sudden disappearance of ice by a theory based upon observations upon a small lake in Connecticut, so hedged in that only the south and south-west winds blow upon it. No large stream feeds it, and its outlet is small. January 23, 1860, the ice was ten or eleven inches thick; the temperature of the ice varied from 34° just below the ice to 43½° at the bottom; average 38⅞°. March 6, the ice disappeared very rapidly, about one-third disappearing during two hours. The mean temperature of the water was then 41½°. He concludes, therefore, that the solution of the ice is caused by heating the water upward from the bottom, since the temperature of the air was less than that of the water.

Professor **Elias Loomis** *on Natural Ice Houses and Frozen Wells*. These occur in places where the ice accumulates in the cold season, and remains during the summer months, or even the entire year, although the mean temperature of the neighbourhood may be 10° or 15° above the freezing point of water. Four such ice caverns are found in Switzerland and the neighbouring portion of France; one being near Besançon. The bottom of the latter cave is covered with ice about a hundred feet square and about a foot thick. There is but one opening to the cave, and so no chance for the circulation of air. The water trickles from the roof or flows in at the mouth, and the cold air which settles in the cave, and freezes all its moisture, maintains its place through the summer by reason of its greater specific gravity, so that the ice wastes very slowly even in the hottest weather. Professor Loomis gave a list of eight such "ice-houses" in different parts of Europe. Similar cases exist in America. On the western bank of Lake Champlain, near the village of Port Henry, is an ice mine which has been extensively worked for many years. There are fifteen such places in the United States. The phenomenon of frozen wells is explained in the same way; but to secure a frozen well, it is necessary that the water should not be changed. It is only the fact that the water in most wells is constantly changing, that prevents all of them from presenting this phenomenon. Professor Loomis produced a list of about thirty frozen wells, the most remarkable of which are, one in Tioga, New York, 77 feet deep; one in Ware, Massachusetts, 38 feet deep; one in Brandon, Vermont, 34 feet deep; six in Owego, New York, from 16 to 30 feet; and one in Prattsburg, New York, 25 feet.

Professor **John Le Conte** read a paper *on the Phenomena presented by the "Silver Spring" in Marion County, Florida*. Although the optical phenomena of this spring had been greatly exaggerated, yet he found, on paying it a visit last December, that it was sufficiently wonderful. While it was reported to be 200 feet deep, a careful measurement showed it to be only 30 feet. On a clear and calm day, the view from the side of a boat is beautiful beyond description. Every feature of the bottom is as clear as if there were no water above it, but only the clear air. The bottom is thickly covered with luxuriant vegetable growth, developed by the large amount of sunlight which penetrates there. Objects beneath the surface of the water, viewed obliquely, appear surrounded by prismatic hues. The beholder seems to be looking down from some high point, upon a truly fairy scene. Large letters at the bottom can be read from the surface as well as if they were in the open air. Small letters cannot be read so easily, because the surface is not entirely quiescent.

Mr H. A. CLUM described an *Improvement in Barometers, of his own invention.* His improvement consists in permanently fixing and adjusting the mean of the mercury, whatever the altitude of the barometer may be. Thus the mean of the mercury at tide-water is 30°; but at 900 feet above tide-water, 29°, so that a common barometer adapted to tide-water would indicate rain (29°) at the elevated position, when it should indicate the mean. Mr Clum's invention adjusts the barometer so that it shall not be a prophet of evil when no evil is impending.

Professor ROGERS also read *some Jottings upon the Geology of the Eastern Part of Maine.* A careful examination of the rocks from Dennis to Perry showed him that there was a striking analogy between the fossil plants of the rock and those of Scotland, Ireland, and other localities near the top of the Old Red Sandstone. It seemed to belong to the upper Devonian where plants begin, and to the lower Silurian.

In Section B, Professor J. D. WHITNEY explained the *Nature of the Lead-bearing Regions of the North-west,* which are so valuable a portion of the country. Ten years ago they furnished one-fourth of all the lead in the world; now, owing to the increased production elsewhere, about one-eighth. Lead is very widely distributed throughout the world, appearing almost always in carboniferous sections. The mines of New England, however, with one exception, have never proved profitable. Professor Whitney urged the importance of keeping a record of all discoveries in mining regions, and of having accurate maps of such regions, for the benefit of future discoverers and miners. The sheet form of lead is the normal deposit, varying from the thickness of a knife-blade to three inches. The longitudinal extension of the sheet varies from one to 100 yards, and in its vertical direction from 20 to 40 feet. In the now abandoned East Black Lead range, sheets were found 140 feet high. He argues that the metalliferous character of these regions in no ways depends upon the azoic rocks beneath; and that they are of aqueous, not igneous origin. He maintained that the elements of the necessary lead salts were present in the water of the original ocean, and that they were precipitated by the sulphuretted hydrogen freely developed by the animal and vegetable matter in the sea.

Professor AGASSIZ explained *the Arrangement of the Museum of Comparative Zoology at Cambridge.* After recapitulating the history of the origin of the Museum, he proceeded to his subject proper. He began by expressing his belief that it was the great misfortune of all such museums, that they are systematically arranged. He had endeavoured especially to avoid the system of the British Museum and the *Jardin des Plantes*, which shows what science has been aiming at in the past. In those museums the student is bewildered, at the outset, by the multitude of animals of one kind arranged together, and before he has done examining one class, he has forgotten that there are any others. We ought to embrace the whole range, and give exclusive preponderance to none. No architect was allowed to spoil the convenience of the building, but it was specially adapted to its use. There was one reception-room, where all specimens are sorted. From these such are selected as are best adapted to exhibit comprehensively the affinities of animals, and a special collection of these is made near the entrance, to give the young student an epitome of the whole science. On the four panels of this room he would exhibit specimens of each of the four great classes of animals, and not only animals of the present period, but the fossils of all periods. But there will also be rooms devoted to a special examination of each class, resembling somewhat the old system, but yet different. It will exhibit the geographical arrangement of animals, showing how they confine themselves to their own regions without aiming at combination or "annexation." He would also have a chronological arrangement, so as not only to group the animals of this period, but those of all epochs. The institution stands already as the ninth in the world. Out of the 8000 species of fishes, the British Museum has 5000, the *Jardin des Plantes* 4000, and the Cambridge Museum 3000: but there are 40,000 specimens at Cambridge, and by exchanging some of these duplicates with the other two great museums, the Cambridge Museum would be at once placed on a par with them in this respect. He hoped to see the day when it would equal them in every respect. He meant to furnish an asylum for the numerous private collections which have been made by laborious students through a long life-time, but for which no public provision had been made.

In section A, Professor WILLIAM B. ROGERS described some *Experiments and Inferences in regard to Binocular Vision.* In the theory of binocular vision expounded by Sir David Brewster, and maintained by Brücke, Prevost, and others, it is contended that no part of an object is seen single and distinctly but that to which the optic axes are for the moment directed, and that "the unity of the perception is obtained by the rapid survey which the eye takes of every part of the object." So that, according to this, our perception of an object in its solidity and relief is acquired not by a simple but by a cumulative process, in which the optic axes are conveyed successively upon every point of the object within view. Like conditions must obviously apply to the perception of the binocular resultant formed by the reverse of the twin pictures of a stereoscope. On this theory the conditions of binocular vision of a perspective line would be as follows:—(1.) The perception of the perspective line in the stereoscope would require the optic axes to be successively directed in such manner as to unite every pair of corresponding points of the two composite lines of the diagram,—or, which amounts to the same, they should be successively conveyed to every point of the perspective resultant; (2.) In cases of two intersecting lines appearing, instead of this single resultant, those lines should neither of them have a perspective position.

In an experimental discussion of the subject some years ago, Professor Rogers showed that the phenomena of the stereoscopic resultant do not necessarily conform to these conditions; and that the perception of a perspective resultant line, or of a physical line, in the same attitude, does not require the successive convergence of the axes to every point. The truth of this position is proved by the fact that the resultant obtained by combining two inclined lines with or without a stereoscope, presents a perspective attitude, even when the component lines, instead of being united into one, are brought together to intersect at a small angle, each of the intersecting lines in this case appearing in relief. Professor Rogers described several experiments, in part new and in part modified repetitions of those already described by Professors Wheatstone and Dove, which offer decisive proof that such a successive combination of pictures, point by point, however it may enter into the complex process of vision, cannot be regarded as an essential condition to the singleness and perspectiveness of the binocular perception. One of these experiments is tried by holding a brilliant line in a perspective position at a convenient distance midway between the eyes, and regarding it for a few seconds so as to produce a lasting impression on the retina. On turning the eyes towards a blank wall or screen, the subjective impression will be seen projected against it and having the same perspective attitude as the original line. If, then, one eye be closed, the line will appear to subside into the surface of the screen, taking an inclined position corresponding to the optical projection of the original line as seen by the unclosed eye, and therefore corresponding to the position of the image formed in that eye. By opening and closing the eyes alternately, and finally directing both to the screen, we are able to see the two oblique lines corresponding to these projections, and their binocular resultant corresponding to the original object. For the success of this and the other experiments described by the Professor, the lines should be very strongly illuminated, and the observer should have some practice in experiments on subjective vision. But the following is a more simple proof that pictures successively impressed on the two eyes are sufficient for the stereoscopic effect:—Let a screen be made to vibrate or revolve somewhat rapidly between the eyes and the twin pictures of a stereoscope, so as alternately to expose and cover each, completely excluding the simultaneous vision of the two. The stereoscopic relief will be as apparent in these conditions as when the moving screen is withdrawn. Here there is no opportunity for the combination of pairs of corresponding points in the two diagrams by the simultaneous convergence of the optic axes through them; but at each moment the actual picture in the one eye and the retained impression in the other, form the elements of the perceptive resultant perceived. In repeating, with success, the curious experiments of Professor Dove, to obtain the stereoscopic effect by the momentary illumination of the electric flash, Professor Rogers found great advantage in using one of Ritchie's improved Ruhmkorff's coils, having a coated jar included in the outer circuit, the intensely brilliant spark of which can be made to throw its light upon the object viewed, in any direction or at any intervals that may be desired. From the facts that the duration of an electric spark is less than one-millionth of a second (Wheatstone), and that we are able by a single flash of lightning to perceive the solidity and relief of an object to which the eyes are directed,—we may conclude that the perception of an object in its proper relief does not necessarily require the eyes to be converged upon every visible point of it in succession, and that the perception of the perceptive resultant, through binocular combination in a stereoscope or otherwise, may arise directly from the two pictures impressed, without the necessity of combining, pair by pair, all the corresponding points of the component lines or drawings. Nor is it necessary that the images of the corresponding points of the objects should fall on what are called corresponding points of the retina. The condition of single vision in this case seems to be simply this, that the pictures in the two eyes shall be such and so placed as to be identical with the pictures which the real object would form if placed at a given distance and in a given attitude before the eyes.

Professor Rogers showed that the law of binocular vision is valuable in examining bank-notes. Put a genuine bill in one compartment of a stereoscope, and a counterfeit bill in another, and every difference will be readily distinguished.

Professor B. Silliman, junr., read a note prepared by Mr C. Lea *on the Sources of Error in the Employment of Picric Acid to detect the Presence of Potash*, giving a caution upon this point to chemists.

Professor Silliman also read a paper *on the Combustion of Wet Fuel*, showing not only how such combustion is possible, but also how it may be accomplished with economical results. A furnace invented by a Mr Thomson in 1854, arrests the escaping products of combustion, and brings them back to consume themselves. The method is a new one, because it shuts off the atmospheric air and obtains the requisite oxygen from the steam.

Captain E. B. Hunt, U.S.A., read a description of a *New Portable Coffer-Dam*, the idea of which occurred to him and was put in practice while superintending certain constructions at Fort Taylor, Key West. The novel feature of this portable coffer is this—Make a strong canvas case for the whole coffer, using two thicknesses of canvas, and interposing a complete coating of mineral tar, to act both as an adhesive and an impervious agent. Along the line corresponding to the bottom of the coffer must be joined a flap, to spread over the bottom as far as may be necessary, according to the nature of the bed of the stream. This bed should be raked clear of sticks and stones. Then the usual process with coffer-dams may be followed. The facility with which the coffer may be taken up and re-established constitutes its great recommendation.

Dr B. A. Gould read a paper *upon the Solar Eclipse of* 1860, prepared by Dr Smallwood of St Martin, C. E. The weather was very favourable for the observation: the sun's disc exhibited several spots, one of large size. The barometer indicated 29·826°; thermometer 62·3; wind S.S.W.; ozonometer ·2 ; electrometer a degree negative ; intensity of the solar ray 65·4. The paper was accompanied by tables, by a curve showing the play of the temperature during the eclipse ; also a photometric scale, showing positive and interesting results ; and photographs of the sun in various stages of the eclipse. During the eclipse the cocks crew and morning-glories drooped. Dr Gould mentioned the fact that the sun's surface was abnormally disturbed for two or three weeks before the eclipse. There were several spots of great beauty upon it.

In Section B, Professor Newberry read a paper upon the *Petroleum* (*or Natural Oil*) *Wells of the Mississippi Valley*. He exhibited specimens of the oil from these wells, which are all upon the same level. The yield of the Pennsylvania wells is from ten to twelve barrels of crude oil a day for each. This oil, in its crude state, is a good lubricator, if mixed with some thicker substance. The oil is generally obtained by pumping, but sometimes the wells are of an Artesian character. They extend, in Pennsylvania, down Oil Creek to the Alleghanies, and from Ohio into the West. Professor Newberry considers these oils as of both animal and vegetable origin. They are deodorised by chemical agents, and are superior to coal oils. Their geological level is in the upper Devonian stratum.

Dr **William P. Blake** made some remarks on the *Distribution of Gold in Veins.* His observations had shown him the fallacy of the common opinion that, if gold is found at one end of a quartz vein it extends through that vein. It follows a general vertical direction, and we should dig down for it instead of lengthwise. One gold mine in Georgia, when dug but 10 feet deep, yielded 10,000 dols.; a single bushel of the blasted rock yielded 3000 dols. He showed some remarkable nuggets from Georgia, quite equal in size and beauty to those from California and Australia. The nuggets came from the Nacoochee mines in Georgia, one of them weighing 387 pennyweights. There was also a quantity of coarse grain, 200 pennyweights, washed out of the soil on the summit of a high ridge. He argued that gold is of igneous origin.

Professor **Daniel Wilson** of Toronto discoursed on the *Ethnological Value of the imitative faculty in relation to the characteristics of ancient and modern American Races.* He argued that it was not necessary to refer the origin of the primitive languages and hieroglyphics of this country to the East. There are inherent characteristics in the hieroglyphics and in the Indian tribes themselves, which indicate that the hieroglyphics were invented here.

Dr Gibbon of N. C., cited several characteristics of the hieroglyphics of Yucatan, tending to show that they were of foreign origin. Professor Agassiz spoke of the value of these investigations, and contradicted the theory that the first spoken language was very imperfect. He had no doubt that the first human beings were fully able to express in words all their feelings, sympathies, and emotions.

Professor J. D. **Whitney** read a paper by E. H. **Bradley**, describing *a new species of Trilobites*, which has just been shown to belong to the genus Conocephalites. Professor Agassiz thought it quite as probable that it was a new generic type of trilobites.

Mr **Nathan** B. **Webster** of Portsmouth, Va., described *Certain Phenomena of the Great Dismal Swamp in Virginia.* It is 21 feet above tide-water mark; is 15 feet deep, with a bottom partly sand and partly mud. Mr Ruffin's examination of the bottom showed that 10 or 15 inches down is a fat, slimy sand, containing about 76 per cent. of vegetable matter.

Professor **Agassiz**, in speaking of *Methods in Zoology*, said that the progress of natural science does not depend so much on our information as upon the methods in which this information is considered and combined. The results of our investigations are acceptable and satisfactorily proved when they stand the tests of criticism. Unhappily the devotees of natural history, still lingering upon the search for facts, have not yet been willing to submit their facts to the tests by which they should be judged. It is the great misfortune of American naturalists that there is so much upon this continent that has not yet been described; all their efforts are directed to discoveries and descriptions, in the belief that in this way glory and fame are only to be obtained. There was a time when this plan was right; now we want something more. We want to arrive at a clearer insight into the foundations of relationship. We must have the means of ascertaining whether our facts are worthy of preservation and record. In some departments of zoology the proper standard has already been obtained. Thus, since the investigations of Germans, transplanted into France and thence into England, there is nobody who does not understand that vertebrates are so different from other classes of animals, that there is no genetic connection between them. He recalled the distinction between vertebrates and articulates, to show that their structural elements are entirely different. Professor Agassiz took up the Radiates, and illustrated upon the blackboard an exact system to which all animals, supposed to be radiates, may be referred as a test. He does not consider the mouth of radiates as corresponding to the mouth of other animals, but only an opening in the cavity of the body, no way analogous to the mouths of other animals. They are often called spheroidal, but they cannot be compared to a sphere because their centre of structure is not the centre of motion. He gives names to the two axes of the animals; that around which the motion of the animal occurs is the actinal axis; its main pole the actinal, and the opposite one the abactinal pole. The diameter in the direction of the motion he calls the cœliacal diameter, and that at right angles to it the diacœliacal diameter. Hereafter we must not take the dictum of Mr X. or Mr Y., that "I hold this animal to be a radiate;" but we will submit it to the test, and if it does not stand the test, we must throw it out.

Professor Bache read another paper upon the *Lunar Diurnal Variation of the Magnetic Declination*, showing that the moon, as well as the sun, affects by a variable amount the direction of the magnetic needle. The deductions of Professor Bache were made from observations at the Philadelphia High School.

Mr C. H. Hitchcock of Amherst, upon the *Geology of Newport*, illustrating a geological map of Rhode Island. The principal formation is the coal measures. There is a large amount of alluvium, filled with pebbles, chiefly of a fine variety of quartz rock, varying in size from the smallest pebble to those 12 feet in length, and having their largest diameters always parallel. He pointed out, also, the localities of granite, slates, dolomite, and serpentine, pointing out the local characteristics of each. Professor Rogers dissented from the theory that the pebbles of the conglomerate receive their peculiar form and shape from pressure while in a plastic state. He referred to the beautiful and wonderful pebbles imported from Maine to pave Washington Street, in Boston, which manifestly were shaped out by the action of water, and agreed that the pebbles in the "pudding-stone" at Purgatory may have been formed in a similar way.

Professor L. E. Chittenden read a paper upon the *Reindeer of North America*, describing their habits, characteristics, &c. It is sometimes said that there are two species of reindeer, one inhabiting the woodlands and the other the barren lands; but Professor Chittenden had no faith in this theory.

A paper investigating the *Problem of a Lunar Tidal Wave on the Great Fresh-Water Lakes of North America*, by Major J. D. Graham, was read by Professor J. D. Whitney. A long series of observations, made by him at both ends of Lake Michigan, determines, after eliminating all disturbing elements, that there is a semi-diurnal lunar tide on that lake, and doubtless also on the other lakes, of at least one-third of a foot. In view of the size of these lakes, this is certainly very probable, but it has remained for the careful observations of Major Graham to establish the fact beyond controversy.

Mr E. B. Elliott of Boston, read a paper giving some *Vital Statistics of the Blind, with an approximate Life-Table.* The statistics of seven State institutions for the blind were given him as data. Out of 1252 cases, of which the condition of 150 was not known in 1859, but of the remainder it was known exactly whether they are alive or dead; viz., 224 had died and 878 survived. Comparing these facts with the results that should be called for by the life-tables of Massachusetts and Great Britain, it appears that there is a deficiency of 8·9 from the former, and 10·3 from the latter, for the period elapsed since admission. He derives a table of the probable duration of life of the blind at various ages, which is given, with the Massachusetts and British life-tables annexed:—

Age.	Prob. Life of Blind.	Do. Mass. Tables.	Do. British Tables.
Birth	32 years.	39·8	41·
10 years	38·1	47·1	47·
20 „	33·2	39·9	40·
30 „	29·1	34·	34·
40 „	24·5	27·9	27·
50 „	19·3	21·3	21·
60 „	13·9	15·	14·
70 „	8·9	9·4	—

From the life-table for the blind, from which he deduces these results, computed by Mr Elliott, annuities may be reckoned, the present worth of dowers estimated, and the same uses made of them for blind people that are made of other tables for those with sight. Mr Elliott attributes the comparative shortness of the life of the blind to several causes; such as a deficiency of vital power; a limitation of the choice of employments and duties, involving sedentary pursuits; and the loss of an important sense, subjecting the blind to accident.

Mr C. H. Hitchcock discussed the *Age of the so-called Taconic Rocks in Vermont.* Professor Emmons maintains that a class of rocks, including the marbles and roofing-slates, belong to another system, below the Silurian, and gives it the name of Taconic. But Mr Hitchcock showed that he had confounded the rocks of two existing systems, and has placed them in the wrong period; in short, that there is no Taconian system, but that Professor Emmons' Taconic rocks belong to upper strata.

In Section A, Professor Bache read some *General Results of the Observations of the Tides at Van Ransellaer Harbour, made by the Second Grinnel Expedition under Dr Kane, during* 1853, 1854, 1855, *from a reduction and discussion by Charles A. Scholt, Assistant in the Coast Survey.* From the tidal motion, the depth of the channel from the southern point of Greenland to Van Ransellaer has been determined at an average of 220 fathoms.

Mr James Hyatt read some *Reflections on the Observations of the*

Solar Spots, and of the Magnetic Variation. He maintains that the variations of the needle show that the earth's magnetism is chiefly due to a solar-thermal effect, and that when the spots upon the sun are at a maximum, the force of the sun is impaired, leaving the needle, under other influences, to sway from east to west. These changes are periodic, increasing and decreasing, like the solar spots, every five years.

Professor WILLIAM B. ROGERS of Boston read a paper *on our Inability from the Retinal Impression alone to determine which Retina is impressed.* Although on first view it might be supposed that an impression made on either eye must necessarily be accompanied by a mental reference to the particular organ impressed, he showed, by describing a few simple experiments, that the impression of itself is not essentially suggestive of the special retinal surface on which it is received. Fix a short tube of black pasteboard, one-fifth of an inch in diameter, in a hole in the centre of a large sheet of the same material; then hold the sheet a few inches in front of the face of a second person, between him and the window, moving it to and fro until the bright circular aperture of the tube is brought in front of one of his eyes—suppose the left eye—and let him fix his attention upon the sky or cloud, to which the tube is directed. He will feel as if the impression or image belongs equally to both eyes, and will be unable to determine which one really receives it. On moving the aperture towards the right, or nearer the nose, but not so far as to be out of the view of the left eye, or to be visible by the right,—the observer will imagine that it is now in front of the right eye, and chiefly seen by it. Shifting it still further in the same direction, until it is brought within the view of the right eye, but not fairly in front, it will appear as if placed before the left eye, and by an additional motion bringing it fairly in front of the right eye, it will seem to be equally before both eyes, or to be in the mesial line between the two. Like effects may be produced by using a half-sheet of rather stiff foolscap with a large pin-hole in the centre; also by fastening a small disc of white paper on a slip of white pasteboard of the size suitable for a stereoscope, and shifting it about in a manner similar to that in the first experiment. From the effect of these experiments, when the object is directly in front of either eye, it may be concluded that the mere retinal impression on either retina is unaccompanied by any consciousness of the special surface impressed; and that the visual perception belongs to that part of the optical apparatus near or within the brain which belongs in common to both eyes. This result also shows that the sense of direction is just as truly normal to the central part of the retina that has received no light, as to that of the retina on which the object has been painted. Indeed, it is normal to neither, but is in the middle line between the two, that is, in the binocular direction. This experiment is at variance with the law of visible direction maintained by Brewster, according to which the apparent direction is always in the normal to the point of the retina impressed. The reference of the object to one eye chiefly, and that the eye not impressed, is due to the direction which the other eye must assume in order to receive the light. In all these cases, indeed, the law of binocular vision comes into play.

Mr W. W. WHEILDON of Charleston, read a paper on the *Open Sea of the Arctic Regions.* He thinks that the arguments in favour of the open sea at the pole being caused by the action of the Gulf-stream, are inadequate; and that while the influence of the Gulf-stream has been exaggerated, that of the air has been overlooked. He concludes that the open sea is due largely, if not entirely, to the currents of air from the equatorial region, which move in the higher strata of the earth's atmosphere, bearing heat and moisture with them. He cites the well-known fact that in the high polar regions the winds blowing from the north and north-east are warm.

Professor E. Pugh, of the Agricultural College in Centre County, Pa., gave a summary of an extensive investigation conducted by himself, with Messrs Lawes and Gilbert, at Rathomsted, England, during the years 1856, 1858, and 1589, upon the assimilation of nitrogen gas by plants. He stated that it had long been admitted that plants could procure this indispensable element from its combination with hydrogen (ammonia), as also from nitric acid, as in saltpetre; and more recently it had been shown that urea, uric acid, and other nitrogen compounds, could afford nitrogen to vegetable organisms during their development. He was of opinion that the more complicated nitrogen compounds, as albumen, were capable of passing into the vegetable organism without being first reduced by decomposition to the state of carbonate of ammonia, as had been generally supposed. But in regard to the assimilation of gaseous nitrogen by plants, opinions and investigations were much at variance. It had been about one hundred years since Ingenhous, Priestley, and De Saussure, had opened the question with discordant results; one supposing that plants assimilated gaseous nitrogen, and formed nitrogen compounds; and the other that not only no such compounds were formed from gaseous nitrogen, but, on the contrary, nitrogenous compounds which plants derived from other sources were decomposed, and free nitrogen evolved from combination.

Dr Draper, of New York, at a recent period, obtained results confirming in the main those of De Saussure, in regard to this question, and finally Boussingault, who took up this question about 1830, and had brought extraordinary skill in careful research to bear upon the question during twenty years' investigation, had to come to a result inconsistent with those of De Saussure and Draper,—or, that plants during healthy growth neither assimilated gaseous nitrogen to form nitrogen compounds, nor decomposed nitrogenous compounds so as to evolve gaseous nitrogen from them; but that during growth the combined nitrogen in connection with the plants did not vary in quantity. But the question did not rest here. With the patronage of the Count De Morny, M. Ville, a few years ago took up this question, and after some time published results entirely irreconcilable with those of Boussingault,—indeed of such a character as to bring to a direct test the respective value of the scientific, if not the moral, character of the two investigators.

Under these circumstances the investigations of Messrs Lawes, Gilbert, and Pugh were undertaken. The results of these investigations were given. They embrace numerous experiments with the ordinary agricultural plants, wheat, oats, barley, beans, peas, clover, buckwheat, and tobacco. The plants were grown from seeds planted in a soil destitute of combined nitrogen, and confined during growth in an atmosphere deprived of combined nitrogen, but supplied with carbonic acid. The plants were all watered with water also deprived of combined nitrogen, so that all sources of it to the plant were cut off, except that afforded by the gaseous nitrogen of the air. If, under these circumstances, the combined nitrogen of the plant increased above that contained in the seed, the plant must have assimilated the nitrogen of the excess thus found. If, on the contrary, no such excess was found, the absence of a power in the plant to assimilate gaseous nitrogen was to be inferred. Extensive investigations went to prove that there was no such gain, and hence no such power to assimilate it was found,—and hence that theonly source of nitrogen to plants was from combined nitrogen.

The experiments were varied by adding small and known quantities of sulphate of ammonia to some plants during growth. This produced great increase of growth, but the sum total of nitrogen found in the plant after growth did not exceed that of the seed plus what was added in the sulphate, and hence no nitrogen was assimilated.

These results were considered decisive in regard to the cereal plants; but owing to the leguminous plants not having grown as well as was desirable, the investigators did not feel fully satisfied as to the result, except, so far as they did obtain results, they went to show that there was no difference between cereal and leguminous plants. Yet they felt that the question required still further investigation, which they expect yet to make.

Among the papers read was one *on Hydraulic Cement*, prepared by Lieut. Q. A. GILMORE, read by Captain HUNT. His deductions from a long series of observations are:—(1.) In cold weather, when it is necessary that the cement should harden quickly, warm water should be used for mixing the mortar and wetting the solid materials with which it is to be used. In warm weather, on the contrary, cool water should be used for the same purpose, in order to delay the setting until the mortar is laid in position. (2.) The time required by a cement to set (if within the ordinary limits of 1·10 of an hour to 1½ hour) furnishes no means of judging of the ultimate strength and hardness which it is likely to attain. (3.) It is not probable that while the present method of manufacturing cement is pursued in this country, we can produce an article equal to Parker's Roman Cement, or the artificial Portland cement from abroad. (4.) The stone furnishing what is generally termed *intermediate* lime, now rejected by our manufacturers as worthless, on account of its containing an excess of caustic lime, may be used with entire safety if combined with 5 or 8 per cent. of an alkaline silicate: "soluble glass" is a good silicate for that purpose. (5.) The maximum adhesion to stone is secured by mixing the cement paste, or mortar, very thin (*en coulis*) rather than very stiff. The maximum density, cohesion and hardness, on the contrary, are all incompatible with this condition. (6.) Cement should be ground to an impalpable powder, when it is intended to give mortar its full dose of sand, the coarse particles of sand being a poor substitute for that article. Finally, all the stone which does not effloresce with dilute hydrochloric acid, or which, during calcination, has been carried beyond the point of complete expulsion of carbonic acid gas, should be rejected.

Professor E. N. HORSFORD read a paper, with quantitative analyses, *upon a new Ammonia Chrome Alum, and the violet, green and red modifications of Chrome Salts.*

Capt. E. B. Hunt, read a paper *upon the Explosions of Fire-damp in Collieries.* He showed that the Davy-lamp had been proved to be unsafe when exposed to a current of gas. He recommends the substitution of coal-gas, manufactured at the surface and near the mouth of the mine, and forced down in pipes, with such gauze safeguards as may be necessary. The advantage of this would be the substitution of a system of lighting, under the care of a responsible person, for dangerous lamps in the hands of the careless and ignorant. Professor Rogers suggested that a very powerful Voltaic light might be used at some point, which, by a series of reflectors, should illuminate the mine. Both he and other gentlemen showed the great danger of employing illuminating gas; although Mr Rockwell of Norwich, Ct., who gave a variety of interesting information upon the subject of mines, showed that the system has been employed, under certain circumstances, in some parts of mines abroad.

In Section A, Rev. H. M. Harmon of Baltimore attempted to estimate the height, velocity, &c., of the meteor of July 20, 1860. He judges it to have been 40 miles high, and to have moved at the rate of 20 or 30 miles a second; and that its diameter was from one-half to two-thirds of a mile.

Dr Benjamin A. Gould read a paper *on the Meteors of August 11, 1859, and July 20, 1860.* He has received but few data of the former meteor; he has 120 accounts of the meteor of July 20, but has not yet sufficiently compared them. He thinks that these bodies revolve about the earth, instead of directly round the sun. He said that a "fire-ball" from the last meteor descended in a yard in Cambridge, cutting off the limb of an apple-tree; this limb was a foot in diameter, and the direction of the fire-ball, which was seen by half-a-dozen persons, was plainly visible in the jagged character of the tear of the limb. A careful search for the fallen fragment was made, but it could not be found, the surface of the yard being covered with chips and rubbish.

Papers, on Induction-Time in Electro-Magnets, by A. D. Bache and J. E. Hilgard; and on the Motions of Uranus, by T. H. Safford, were also read; also a purely-magnetical paper, upon the Possibility of Expressing the Polar Co-ordinates of the Asteroids by converging series admitting tabulation.

Dr Gould read a paper *on Certain Variable Stars of a very Minute Magnitude,* of which about eighty have been noted. Ten or twelve astronomers in this country are devoting their especial attention to this class of bodies, and are as well rewarded as the searchers for asteroids. Some astronomers hold that every star above the third magnitude is variable.

Professor Bache presented an abstract of the principal results of the astronomical observations at Van Rensellaer Harbour and other places near the north-west coast of Greenland, made by Dr Kane's second expedition, from a discussion and reduction by Charles A. Schott, assistant U. S. Coast Survey.

Professor J. D. Whitney read a paper *on the Origin of the Prairies* *of the North-west,*—especially of Michigan, Wisconsin, Illinois and Iowa,—the prevailing character of which is level. They first appear, somewhat, in Ohio; more in Indiana; but the Prairie State, *par excellence,* is Wisconsin. There are two or three kinds of prairies,—the cotton prairie is found only in the Mississippi River. The popular belief is that the prairies are caused by the loss of the trees by fire—an untenable theory, because the ground would of course be as level when covered with trees. Some say that the trees have been so often burned by Indians, that they no longer dare to grow! Others say that the want of rain prevents trees from growing, but no such cause exists. The real cause of the absence of trees, he concludes, is in the mechanical condition of the soil, which is exceedingly fine and comminuted. Trees require a coarse, rocky soil. This is the prevailing cause. In some places, however, the cause is an excess of moisture; in others, an alternation from excessive wet to excessive drought.

Professor Clum showed that no water can be found, as a general thing, upon the prairies without digging 100 to 300 feet for it; while on the woodlands it was not more than about 19 feet deep. Professor Newberry corroborated generally the statements of both gentlemen.

Professor ALEXANDER *on the Results of the Astronomical Expedition to Labrador to view the Eclipse.*—This expedition went out under the direction of the United States Coast Survey. It left New York June 28th, in the surveying steamer Bibb, Lieut. Murray, United States Navy, commanding. The general charge was given to Professor Stephen Alexander of Princeton, New Jersey, who was aided by President F. A. P. Barnard of the University of Mississippi; Professor Smith of Annapolis; Messrs S. Walker and E. Goodfellow of the Coast Survey (who had charge of the magnetic observations); Professor Hannibal, and Mr Lieber of Columbia, South Carolina; Lieut. Ash of the British Navy; P. C. Duchocois, photographer, of New York; and Mr Thompson of the Coast Survey, who assisted the photographer. When perfectly sure that the exact minute of first contact had arrived, Professor Alexander gave the signal, and the picture was taken. It proved that the belt of light nearest the moon was much brighter than the rest. Along the edge of the moon was a bluish light, which the photograph caught. Before, it has been thought that this light was only in the observer's eye, but now it is proved that there is something there, which must be studied hereafter. No wonder that an eclipse used to be supposed to be caused by the devouring of the sun by a wild animal, or by the sun's slow fading away! The phenomenon was a most beautiful sight, and it was with difficulty that the beholders could restrain their ecstasy. It looked like an intensely brilliant, incandescent fragment of metal exposed to the intensest heat, the sharp points falling away until the sun was gone. But the clouds prevented a thorough observation. It was the especial duty of one of the officers of the ship to watch the shadow of the moon as it passed away upon the cliff: it came with fearful velocity, and was gone in three minutes. When the shadow was upon the observers, they saw an intensely beautiful array of colours – copper, leaden, golden, and ruddy. Below, the bold cliffs were of a dark bluish-green. The whole spectacle was grand and beautiful. A newspaper, to be read, must be brought within four inches of the eye, the light being much less than the twilight at midnight. Just after the eclipse became total, Lieut. Ash caught a slight view of the small light blaze. This observation is indeed valuable. But they did not quite lose the corona. Professor Alexander had arranged a number of observations for the seamen, under the general charge of the commander. He prepared a set of simple questions, and from the intelligent quartermaster he gathered the fact of how they saw the corona, how it looked and trembled, and how it shot out. The description was natural as precise. Professor Alexander fixed a prepared sheet, in accordance with the ideas which he gathered, correcting it until the quartermaster and the seamen said that it was exact.

President BARNARD then gave the physical view of the question. He regretted that the state of the atmosphere had prevented them from observing several phenomena, and hoped that observers in other quarters had been more successful. He saw that curious breaking up of the lines of light between the sun and moon just at the moment of total obscuration, described by Francis Bailey in 1837, and called "Bailey's beads." They also saw the phenomenon, but the fragments did not present that rotundity which would entitle them to be called beads. Their appearance, he said, was so beautiful that the chief of the expedition was carried away, and, forgetting himself, broke the law of silence by exclaiming, "Bailey's beads!" Dr Barnard said that, at the time of the phenomenon, he was trying to count the number of beads, and did not see the phenomenon observed by Lieut. Ash. Previously to the meeting of the two limbs, it had been noticed that the moon's edge was very rough, while the sun was smooth. They saw nothing of that drawing out of the beads, and their breaking as if they were the filaments of a viscous fluid. Bailey himself did not see them, when in Italy in 1842. Dr Barnard thinks the beads are owing to the irregularity of the lunar disc at the edge where it meets the solar edge. On passing the Straits of Belle Isle, July 7th, the expedition met with a series of optical phenomena. The irregular refractions of light were exhibited on a large scale all around the horizon, constantly changing. He analyzed the three kinds of mirage, specimens of all of which they saw. The icebergs, under this refraction, presented a constantly changing aspect, extremely interesting: sometimes the true, sometimes the false, image was greatly exaggerated. Sometimes the phenomenon lasted so long that they could not take correct views of the coast. The company observed fifteen auroras: those in the high latitudes were chiefly coronas; a number of them very lasting, but not very dense.

Professor Alexander spoke of the magnetic variations, which were extreme. Under the influence of the eclipse, however, they were quiet.

Botanical Society of Edinburgh.

Thursday, 14*th June* 1860.—Professor ALLMAN, President, in the Chair.

The following communications were read:—

I.—*On the Effects of Anæsthetic Agents on Sensitive Plants.* By Mr WILLIAM COLDSTREAM.

(This paper appeared in the last number of the Journal.)

II.—*Account of a Trip to Clova, with Pupils, in August* 1859. By Professor BALFOUR.

The party, consisting of Dr Balfour, Mr M'Nab, Dr Pougnet, and Messrs A. Graham, Linton, John Rutherford, Le Déaut, Labonté, T. Pougnet, Branch, Corlett, and Bell, left Edinburgh on 12th August 1859, and proceeded to Kirriemuir and Clova, where they took up their quarters for about a week. They were afterwards joined by Mr Barclay. The use of the hall at Clova was kindly granted by the Hon. Donald Ogilvie. The party examined Glen Dole, Glen Fee, the banks of the Dole, and White Water, Little Gilrannoch, Loch Brandy, and the various mountains in the vicinity, and some of them proceeded afterwards to Ballater and the banks of the Dee. Among the rare plants collected may be noticed the following:—*Cherleria sedoides*, *Lychnis alpina*, *Astragalus alpinus*, *Oxytropis campestris*, *Dryas octopetala*, *Potentilla alpestris*, *Saxifraga nivalis*, *Erigeron alpinus*, *Gnaphalium supinum*, *Mulgedium alpinum*, *Saussurea alpina*, *Azalea procumbens*, *Veronica alpina*, *V. saxatilis*, *Salix lanata*, *S. Lapponum*, *S. reticulata*, *Betula nana*, *Gymnadenia albida*, *Malaxis paludosa*, *Juncus castaneus*, *Luzula spicata*, *Carex aquatilis*, *C. atrata*, *C. pulla*, *C. rariflora*, *C. vaginata*, *C. Vahlii*, *Alopecurus alpinus*, *Phleum commutatum*, *Poa alpina*, and *var. vivipara*, *Polystichum Lonchitis*, *Pseudathyrium alpestre*, *P. flexile*, *Isoetes lacustris*, *Lycopodium annotinum*,*Splachnum mnioides*. On the banks of the Dee, near Ballater, which is 700 feet above the sea, *Aquilegia vulgaris* was gathered; near Monaltrie, *Linaria repens;* and near Pannanich Wells abundance of *Mimulus luteus*. The Serpentine hills of Coial were visited, and on them were gathered *Armeria maritima*, alpine form; *Silene maritima*, alpine form; *Anthyllis Vulneraria* in a very small state; *Arabis petræa*, and a peculiar form of *Saxifraga hypnoides*. On Loch-na-gar *Saxifraga rivularis* was gathered. At Loch Muick, which is 1200 feet above the sea-level, the holly was found growing well. In many places the peculiar twisting of the wood of the Scotch fir was noticed. On an island in the Loch of Kinnord, *Conium maculatum* and *Digitalis purpurea* were seen; and in the lake, *Lobelia Dortmanna*, *Nymphæa alba*, *Nuphar lutea*, and *Littorella lacustris*. In the same district *Radiola Millegrana* was seen. It was remarked that many plants were seen flowering a second time.

A specimen of the Kola-nut from Sierra Leone (*Sterculia tomentosa*) was exhibited, which had been transmitted by Mr Baillie from Mr George Thomson. In the note accompanying the nut, Mr Thomson says—"It is held in great estimation by the natives in the neighbourhood of Sierra Leone, especially by the Mohammedans, who call it the 'blessed Kola,' and consider it to be the veritable forbidden fruit. In the interior of Africa, it is scarce, and is so much prized that five Kolas are said to be equal to the price of a slave. I understand that it is much used as a substance for chewing, and is said to possess the property of keeping away the craving of hunger to such a degree, that a man can travel for many days without anything more than a single Kola. It will be observed how curiously the two halves of the bean lock into each other. This peculiarity is noticed in the following African fairy tale. A Criffy or one of the Genii, who has married a young and interesting lady, having occasion shortly after their happy union to go on a long journey, separates a Kola, retaining the one half to himself and presenting the other to his spouse, with strict injunctions to keep it carefully, and threatening some fearful doom, should she on his return be unable to produce it. She is consequently very careful for a time, but is ultimately deprived of it by an envious sister, and you may easily imagine the dismay of the poor girl on discovering her loss, there being no possibility of obtaining any other half Kola that will tally with the one in possession of her lord."

Colonel Maclean, Royal Artillery, sent for exhibition 67 drawings of Chinese plants, executed under his direction by native artists at Hongkong. Mr M'Nab placed on the table plants of *Lychnis alpina* exhibiting marked variations under cultivation, also a hybrid plant of *Papaver*, betwixt *P. nudicaule* and *P. alpinum.*

Mr Robert Brown exhibited a piece of wood taken from the supports of the mud barge at Granton Harbour, pierced by *Limnoria terebrans*, a sessile-eyed crustacean. It seems also to be attacking the piles which support the Chain Pier at Trinity up to highwater point. Though it does not bore deep, yet, by disentegrating the wood on the outside, it exposes the structure to the action of the waves, so that the supports of the Chain Pier by this means are in some places, notwithstanding the iron sheathing, almost eaten through. Its effects are also visible at Granton. Its ravages are thus dangerously destructive, and, from the small places into which it can insinuate itself, more dangerous than either Teredo or Pholas.

Thursday, 12th July 1860.—Professor Balfour, V.P., in the Chair.

The following Communications were submitted to the meeting:—

I.—*Vegetable Morphology; its general Principles.* By the Rev. Dr Macvicar, Moffat.

(This paper appears in the present number of the Journal.)

II.—*Observations on some Bisexual Cones occurring in the Spruce Fir.* By Alex. Dickson, Esq.

(This paper appears in the present number of the Journal.)

III.—*On the Movements in the Cells of Anacharis and Vallisneria.* By S. J. Meintjes, Jun., Esq.

The author stated that the cells of this plant are brick-shaped, and formed of a single wall. The marginal cells contain less granular matter than those nearer the midrib. These granules, when in motion, line the circumference of the cell, never actually touching the wall. When in a favourable state the granules move regularly round the cells—some in one direction, others in an opposite one. The movement is a slow, rolling motion along the sides of the cell; when the angles of the cells are acute the granules are jerked across, and on regaining the opposite side resume their steady progressive movement. Sometimes a granule is sent out with a jerk from the true course, but it immediately returns, and continues its course. All these phenomena are seen under a power of about 300. When a higher power (650) is used, a thin wave-like line is seen lining the whole of the cell, and in actual contact with the granules when present. This at first seemed an optical delusion, but repeated examinations plainly showed this line to be the free margin of a row of cilia. F. Branson, in a paper published in one of the numbers of the *Microscopical Journal*, pointed out this some time ago, and without being at the time aware of Mr Branson's paper, Mr M. verified all his statements. The conclusion he arrived at is, that the motion of the granules is due to the presence of cilia. The way in which these cilia act is peculiar. Being set at an angle to the cell wall, their action takes place in the direction of their angle;

when set in motion they create a strong eddy in the cell. This eddy, by its centrifugal force, drives the granules to the side of the cell, and, coming under the influence of the cilia, they are carried along the side. Owing to the shape of some of the cells, a secondary current is produced in the acute angles, and, when the granules come under the influence of the two currents, they are thus rapidly conveyed across the angle—hence the observed jerk. Another effect of the irregularity of the shape in the cells is, that the eddy can never be perfectly circular, and, this being the case, its force cannot be equally distributed. This inequality causes the outer edge of the eddy to act with greater force on some parts of the cell than on others, and the lighter granules, being sent with a greater force against those parts, rebound. The elasticity of the cilia increases the repulsive power, and also the force of the rebound caused by the action of the eddy.

IV.—*Account of Professor Balfour's Botanical Trip, with Pupils, to Moncreiffe and Kinnoull Hills in Perthshire.* By Mr JOHN SADLER.

About 160 botanical students left Edinburgh on the morning of 16th June last for the Bridge of Earn; whence, after breakfasting, they walked by Moncreiffe, Kinfauns, and Kinnoull to Perth.

The principal plants observed by the party were—*Nymphæa alba*, *Montia fontana*, *Villarsia nymphæoides*, *Lemna trisulca*, *Alisma Plantago*, at Moncreiffe Loch; *Epipactis latifolia*, *Hesperis matronalis*, *Scrophularia vernalis*, *Symphytum tuberosum*, *Erodium cicutarium*, *Geranium columbinum*, *Geranium sanguineum*, *Peucedanum Ostruthium*, *Mimulus luteus*, and *Mentha sylvestris* var. *velutina*, in the woods and on the Hill of Moncreiffe; *Lactuca virosa*, *Dipsacus sylvestris*, *Hesperis matronalis*, *Cheiranthus Cheiri*, *Sedum Telephium*, *Fumaria micrantha*, *Malva moschata*, *M. rotundifolia*, *Geranium pyrenaicum*, *Myrrhis odorata*, *Trifolium arvense*, *T. striatum*, *Potentilla argentea*, *Valerianella Olitoria*, *Poterium Sanguisorba*, *Rosa systyla*, *R. villosa*, *Cynoglossum sylvaticum*, *Lamium maculatum*, *Inula Helenium*, &c., at Kinnoull, and near Bridge End.

V.—*On the Structure and Development of* Botrydium granulatum. By GEORGE LAWSON, Ph.D., Professor of Chemistry and Natural History in Queen's College, Kingston, Canada.

(This paper appears in the present number of the Journal.)

VI.—*On the Effects of Lightning on an Ash-Tree.* By Dr JOHN ALEXANDER SMITH.

Mr Smith remarked—"I send a splinter and piece of bark of an ash-tree, which may perhaps be considered worthy of a place in the Botanical Museum. The tree was demolished by lightning on Saturday the 16th June. I examined the tree and found it had been about two feet in diameter at the base, and formed one of a long row of old trees of a similar kind (none of which were injured except itself) on the farm of Hollydean, in the parish of Bowden, Roxburghshire. It had been struck apparently at the upper part of the trunk (the branches not being stripped of their bark), and the tree was cloven in two to the very root, two stumps only remaining in the ground, and these were shattered again in a lateral direction, as if the bolt had exploded in the tree, and blown the trunk into numerous fragments, some of the pieces being picked up at the distance of fifty yards or more. The wood of the tree seemed quite sound, and the fragments, when examined, were apparently perfectly dry and sapless. Could the sap of the tree (for it was just coming into full leaf, and therefore full of sap), by its rapid conversion into vapour by the electric fluid, have any share in splitting the tree so effectually into fragments? The tree was cloven to the ground, one-half of the root itself being raised out of the earth, and the ground was ploughed up in a straight line for ten paces to the north, and the same distance to the south of the riven stump of the tree. This adds another instance to the list of ash-trees struck by lightning. No doubt the ash is a common hedgerow tree; still it seems to have rather a dangerous attraction for the electric fluid."

VII.—*Notice of some Plants, specially Orchids, found in Kent by* G. CHICHESTER OXENDEN, Esq.

Mr Oxenden says:—"I have seen some very fine sights this May and June—namely, vast tracts of steep picturesque grass hills extending for some miles, and throughout their whole length decked and garnished with one or other of the following plants:—*Ophrys aranifera* and *muscifera*, *Orchis ustulata*, a lovely orchid, and *Habenaria bifolia*—all this vast range of hill slopes to the south and south-west. The east side of the same range is all forest ground, and it affords in abundance every variety of *Orchis fusca*, from a dull white to a very deep mulberry colour, and in size over twenty inches. These same woody banks yield a few specimens of the strange *Lathræa squamaria*, and more to the eastward I find the truly curious *Monotropa Hypopitys*. Near the place from which I write (Broome Park, near Canterbury), grows the monarch of orchids, *Orchis hircina*, the lizard orchid; and within fifty yards of my house I have one growing which at this moment (25th June 1860) is 29½ inches high, and with nearly 50 "lizards" upon it. Next month (July) will afford me very fine specimens of *Ophrys arachnites*; and if you have never seen the wonderful varieties of this orchid, they will astonish you. Some of the varieties of the Bee Orchis are also exceedingly curious. In August we get *Herminium Monorchis* in abundance, very minute, very fragrant, and under the microscope the most beautiful object imaginable. In July and August we have *Epipactis latifolia*, and *E. purpurata* in tolerable abundance."

VIII.—*On the Stem or Axis as the Fundamental Organ in the Vegetable Structure.* By CHRISTOPHER DRESSER, Ph.D., Lecturer on Botany, London. Communicated by Professor BALFOUR.

The doctrine of Goethe relative to the nature of the floral parts, introduced into science by Jussieu and De Candolle, has, by its maturation under the most favourable circumstances, resulted in the conclusion, that only two fundamental organs exist in plants—the axis and the leaf. This conclusion has been very happy for botany; for upon the reduction of a science towards simplicity, we generally have a corresponding extension of its higher and more general principles. But when we carefully view the position of our science in relation to innumerable incidents which are continually occurring, it behoves us to inquire seriously as to whether we have yet fully solved the question relative to the fundamental organs of plants, and whether we are right in referring certain organs of plants to these types, at least in the manner that we do. To this subject I have already called the attention of the Society, in a paper which I presented in November last, wherein I expressed certain opinions relative to the morphology of the flower, which to my mind have a most intimate association with our knowledge of the ovule, and of embryogeny. Having thus expressed an opinion as to one cause of our not having a more definite knowledge of the facts connected with the fertilisation of the ovule, I proceed to a second point, which also stands in close relation to these facts.

I have stated that all vegetable organs are referrible to two types, the foliaceous and the axial; and now I have to offer my firm conviction that there is but one fundamental organ in the vegetable structure, viz. the stem or axis. In order to understand the nature of the case now presented, it is necessary that we should mark the distinctive characters of the stem and leaf:—

1st, The axis is the pre-existing organ by which the leaf is given off.

2d, The leaf can only be formed by an axis; hence it always proceeds from such, and is younger than the axis by which it is developed.

3d, The stem grows primarily by the formation of cells at its summit.

4th, The leaf grows by the formation of cells at its base.

5th, It follows as a corollary, that the oldest portion of a stem is its base, and of a leaf its apex.

To my mind, the leaf is simply a branch with a retrograde development, which position I will endeavour to establish in few words; but in doing this I shall continually have to refer to the stoppage of growth in certain directions, as occurs in the definite axis, which stoppage I shall, for the sake of convenience, attribute to a quasi-paralysis of the growing-point, as I am totally unacquainted with its real cause.

In order to establish my position, it is necessary that we inquire into the result of the quasi-paralysis of the summit of an axial organ, and here we notice that new cells cease, at this point, to be formed; but mark, the stem does not at once cease to elongate, at least necessarily, for new cells are still formed in the internodes, by which it becomes extended, and thus the quasi-paralysed apex of the stem is raised, for the stem is not solely elongated by the formation of cells at its apex, but also, by the development of utricles, for a given time, throughout its entire length.

A similar thing occurs in the case of the leaf, where the latter is a branch which is quasi-paralysed, for which reason it is a body which does not grow at its apex. And mark the manner in which the paralysis proceeds. It first takes place at the apex of the leaf, and then passes gradually downwards in the manner that the sensation passes down the leaf of the *Mimosa pudica,* when the terminal leaflets are cut; thus the cell formation is first arrested at the summit of this organ, and then consecutively lower. This explanation will fully account for the reverse mode of growth of the leaf. The active cell-formative power of the plant resides near the periphery of the axis, and this would also account for the growth of the leaf taking place at its base when its summit is quasi-paralysed; and light would seem to be thrown upon this by the fact that the apex of the leaf, when removed from the stem, ceases to grow, a circumstance which was long since pointed out.

The leaf appears to be a stem, the growth of which is limited or definite, and which is formed (speaking according to appearances) in a retrograde or backward manner. That the leaf is a stem appears to be proved by its being possessed of nodes, as seen in *Bryophyllum calycinum;* and that the crenatures of the margin of the leaf of the *Bryophyllum* are nodes is proved by these points giving off regular buds. Also, adventitious buds are alike given off by stems and by leaves (that the leaves of *Begonias* and *Crassulas* have this power to a remarkable extent has lately been proved). And we have intermediate forms between the axis and the leaf, as are presented by the phyllous branches of *Xylophylla* and *Ruscus.* But the point which will most fully establish my position, if it can be completely worked out (and my firm conviction is that it will, as soon as we have made more observations on the subject), is, that a second layer of wood is deposited in the perennial leaves of Pinus, Abies, &c., which Schleiden is fully persuaded he has already traced.

While I regard the stem and leaf as modifications of one organ, yet I of course admit that there are both leaf and stem, which are each characterised by particular habits, in the same manner that common and allatropic phosphorus are conditions of the same element, yet I consider that, between the leaf and the stem we have a great number of transitionary bodies, which establish the relation between them which I have endeavoured to express.

Mr Scot Skirving sent a specimen of grass which had been observed by Dr Scott of Her Majesty's 79th Regiment, when in the Crimea, to withstand the utmost rigour of the severe winter there. He states that cattle ate it, and fattened on it. The seeds had been sown in East Lothian, and had produced an abundant crop. The plant was *Bromus maximus.*

Dr Rorie, of the Dundee Asylum, sent specimen of cabbage leaves exhibiting peculiar hollow pitcher-like appendages at their extremities.

Mr Guthrie, of the "North British Agriculturist," sent a specimen of a plant from St George's Sound, Australia, which was said to be highly poisonous to cattle and sheep, who partake of it readily. The plant seemed to be *Gastrolobium obtusum.*

Dr Balfour exhibited, from Dr Christopher Dresser, specimens of monstrosities in the flower of the *Passiflora cærulea*—the parts of the ovary being converted into stamens.

Dr Balfour also exhibited a specimen of *Rhodymenia cristata*, Grev., found by Mr Charles W. Peach in Wick Bay, June 1854, the same locality in which the plant was obtained by Borrer and Hooker many years ago.

Mr R. M. Stark exhibited several varieties of British ferns, including *Athyrium Filix-fœmina* var. *plumosum.*

Mr M'Nab placed on the table a complete series of species and varieties of British ferns.

Mr Archer exhibited a peculiar pipe from Zambesi, used for smoking, sent by Dr Kirk.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Notice of a Form of Paralysis of the Lower Extremities, extensively prevailing in part of the District of Allahabad, produced by the use of Lathyrus sativus *as an Article of Food.* By James Irving, M.D., Civil Surgeon of Allahabad.—In October 1856, Mr Court, the Collector of Allahabad, when in Pergunnah Barra, on the right bank of the Jumna, was very forcibly struck by the number of lame persons whom he met in all directions. On inquiry he found, in village after village, that there were several cripples in each. He was also informed that the disease which gave rise to this lameness was of recent origin, and that it was attributed by some of the people to their living on bread made from *kessaree dâl*, and by others of them to the unwholesome qualities of the wind and water of the Pergunnah; the latter being vague causes of disease ever ready to be brought forward by the natives in order to account for any unusual or unintelligible sickness. Several cases of paralysis of the lower limbs were sent from Barra to the Government Charitable Dispensary at Allahabad for medical treatment. Unfortunately, however, they got tired of the means employed for their cure, and left after being in hospital for a month or five weeks. But, through the kindness of Mr Court, who accompanied me to Barra, I was enabled to make some few inquiries into the nature and history of the malady.

Close to the village of Kheerut Gohanee, on the Sohagee Road, all the lame people from surrounding villages were mustered for my inspection on the morning of the 6th February 1857. About fifty men were present, all more or less lame in both legs; some so much disabled as to be hardly capable of motion, while others were only slightly affected. One after another was questioned, and the following particulars were thus gathered. Without exception, they all stated that they had become paralytic during the rains; in most cases suddenly so; and several stated that it had been during the night. Men who had gone to bed quite well, had awoke in the morning feeling their legs stiff and their loins weak, and, from that day, they had never regained the use of their limbs. At first the lameness was trifling, and amounted only to unsteadiness of gait, and slight stiffness chiefly of the knees. After a time the muscles of the thighs commenced to ache and feel weak, and also the loins. In no case did those examined admit that they had then, or ever had, severe pain either in their limbs or loins. They all ascribed their disease to their feeding principally on *kessaree dâl*, but they seemed to imagine that, in order to produce the malady, there must be another circumstance superadded, viz., the deleterious quality of the water during the rains. So far as could be gathered, it was not from drinking the water that they fancied they took harm, but from getting wet by it. More than one dwelt on the fact of his having been exposed to rain either while ploughing or tending sheep; and others spoke of having been working in jheels just before they became lame, at various periods embraced between the months of July and October. The people were particularly examined, and questioned as to whether they had had any symptoms of fever, or of any other disease at the time that they lost the use of their limbs; but they all said that they had not, and nothing was discovered to lead to the inference that this was not strictly true. In only one of many cases examined was enlargement of the spleen observed. Many of the men appeared to be strong looking, and their legs even, in most cases, did not seem to be much wasted, if at all so. It was stated by those affected, as well as by several native officials who were interrogated on the subject, that the complaint did not lead to other diseases, nor tend to shorten life, unless indirectly by preventing the individual working, and thus procuring proper means of support. It was further stated, that the arms were never affected; but that there were some few cases of persons so greatly crippled that they could not walk. It was added, that males were more often afflicted than females; and that ryots were more liable to the disease than the zemindars, although the latter class was not exempt from it. . .

The paralytic symptoms which prevail so extensively in Barra are, by the natives, very generally attributed to their making large use of *kessaree dâl*, the *Lathyrus sativus* of English botanists; and it is perhaps one of the most remarkable circumstances connected with the malady, that the people should be so fully persuaded that in eating this grain they eat poison, and that yet notwithstanding they have continued, and will continue to do so, from generation to generation. *Kessaree dâl* is not unlike gram, and is common enough in most parts of India. It is frequently sown along with wheat or barley, and cut green as fodder for cattle. In Barra the *kessaree dâl* is ground and made into bread. It is sometimes mixed with other grains, such as barley; but is more generally taken alone, the people, in fact, not being able to afford anything else. It is the cheapest grain procurable, and forms the chief support of the people from March till October. On the 7th February 1857, in the bazaar of Barra, wheat sold at the rate of fourteen seers to the rupee, while *kessaree dâl* was at the rate of twenty-two per rupee. It grows without labour or trouble, and on damp swampy ground that will bear no other crops. The land is merely ploughed slightly once, and the seed thrown in; or the plant sows its own seed, which germinates freely next year without further attention or care.

In Europe also paralysis of the lower limbs has been observed to follow the use of *Lathyrus sativus* as an article of food. Thus Don, in the "Gardener's Dictionary" says, that the flour of this plant, mixed with wheat flour in half the quantity, makes very good bread, but alone produces surprising rigidity of the limbs in those who use it for a continuance. In the same quarter of the globe similar effects have also been observed to follow the eating of other kinds of grain produced by the same great natural order of plants, the Leguminosæ—to which the *Lathyrus sativus* belongs; as well as other species of the same genus. Thus Dr Taylor alludes to *Lathyrus Cicera* and *Ervum Ervilia* (bitter vetch), as occasionally rendering bread poisonous. In some parts of the Continent, a bread is made from the flour of the *Lathyrus*, which is so injurious in its effects, that the use of it has frequently caused its prohibition by law. Loudon states, that when mixed in equal parts with wheaten flour it makes a good-looking bread, which, however, occasionally gives rise to weakness of the knees, and spasmodic contractions of the muscles. Cattle and birds, when fed on the seeds, are said to become paralysed. A more recent example of the poisonous effects of *Lathyrus Cicera* flour is furnished by M. Vilmorin; he remarked that "the use of this bread for a few weeks produced complete paralysis of the lower extremities in a young and healthy man. Six or seven individuals of the same family, who had eaten it, suffered more or less from similar symptoms, and one had died. A physician who practised in the district remarked, that paralytic affections were very common among the poor, who subsisted on this bread, while they rarely occurred among the better classes. When the *Lathyrus* flour formed one-twelfth part, no inconvenience was observed to attend its use; in a proportion greater than this it becomes injurious; and when it amounted to one-third part, the effects might be serious." (Annales d' Hygiène, Avril 1847, p. 469—Taylor on Poisons, p. 536). Dr Lindley also states, that the seeds of *Ervum Ervilia*, mixed with flour and made into bread, produce weakness of the extremities, especially of the lower limbs, and render horses almost paralytic. ("Vegetable Kingdom," 2d Edit. p. 548).

As to the treatment of cases of paralysis caused by the use of *Lathyrus sativus*, I have little to say from practical experience. About a dozen cases have come under my observation at the Dispensary, but most of them disliked the restraint and the means of cure employed, and left after they had been patients for a month or five weeks. In some strychnine was tried; in others blisters to the loins frequently repeated; in others tonics; to all I gave generous diet. Two seemed to be somewhat benefited, and could walk better; and in one case the improvement was such, that a man who formerly could only walk with the aid of two sticks, could after a time proceed without any assistance. He was under treatment at the time of the rebellion in June 1857, when the Dispensary was burnt down by the "*poor natives*," for whose use it had been built and maintained by Government. What seemed to me of most use were tonics and generous diet, together with the application of occasional blisters.

The natives of Barra do not appear to have any kind of rational treatment. They rub the lower extremities with various liniments, of which one is composed of oil, garlic juice, and opium. They fancy that eating pigeon's flesh is of use. It was stated to Mr Court that this affection was of recent origin in Barra; but on asking a native official who had known the Pergunnah for twenty years past, I was informed that the disease had, to his knowledge, always existed; although he thought that of late it had become more common; and villages in which formerly there were no cripples now contained several.—*Indian Annals of Med. Science.*

Phipson on the Presence of Aniline in certain Fungi.—Several fungi belonging to the genus *Boletus* possess the remarkable property of changing colour when we press their tissue. Their internal cellular tissue, at first colourless, assumes in these circumstances a very lively colour, which, however, is evanescent, and differs according to the species. This phenomenon is well seen in *Boletus cyanescens* and *Boletus luridus*, the internal tissue of which, when exposed to the air, becomes of a beautiful indigo tint. The colouring matter which is present in these *Boleti* in their uncoloured state is soluble in alcohol, is with difficulty mixable in water, and becomes resinified in the air. It possesses the properties of aniline, and, with oxydising agents, gives rise to the same coloration as aniline and its saline combinations.

On the Ancient Vegetation of North America. By Dr J. S. NEWBERRY.—The general results of Dr Newberry's observations on the extinct floras of North America may be very briefly stated as follows:—

1*st*, The flora of the Devonian and Carboniferous epochs in America was, in all its general aspects, similar to that of the Old World, which has been so fully described; most of the genera, and a larger number of species than at any subsequent period, having been common to the two sides of the Atlantic. The relative number of identical species has, however, it seems to me, been somewhat overrated. In many of the species regarded as the same in Europe and America, the American plants present prevalent or constant characters which may serve to distinguish them. These differences, though frequently remarked by writers, have not been thought to have a specific value; yet it is quite certain that they are as tangible and important as those which now separate many American and European species of recent plants and recent or fossil animals. I have a conviction that the progress of science will considerably diminish the proportion of identical species; a closer scrutiny and more extensive comparison of specimens resulting in the discovery of constant, though inconspicuous characters, which shall be ultimately conceded to be specific.

It is true, also, that in molluscous palæontology, recent geology, and botany, the number of species common to the two continents has been considerably reduced of late years; a large number of American representatives of European species, at first considered identical from their striking and obvious coincidences, having, on closer study, afforded constant though less conspicuous differences.

2*d*, The Permian, Triassic and Jurassic rocks have hitherto furnished us but few species for comparison, but the material is increasing, and I have now on hand a large collection which has not yet been studied. Enough is already known to show that the great revolution which took place in Europe at the close of the Permian epoch was matched by a parallel though less sudden change in the flora of America.

Here as there the Lepidodendroid trees, the *Sigillariæ*, the *Næggerathiæ*, the *Asterophyllitæ*, and the great variety of ferns that gave character to the Carboniferous vegetation, were superseded by *Voltzia*, *Tæniopteris*, *Camptopteris* and a varied and beautiful Cycadaceous flora, in which were many species of *Zamites*, *Pterophyllum*, *Nilssonia*, &c., the representatives of those of the "Age of Gymnosperms," which culminated in the Jurassic epoch of Europe.

During this great interval the generic correspondence between the floras of Europe and America was perhaps as plainly marked as during the Carboniferous age, but the relative number of identical species was apparently smaller.

3d, At the commencement of the Cretaceous epoch the flora of the continent was again revolutionized, and the vegetation of its temperate portions given the general aspect that it now presents.

This statement will surprise many, for the flora generally ascribed to the Chalk period is greatly different from that of the present. Unger has thus represented it, and Brongniart calls it a transition from the great Cycadaceous flora of the Jurassic period, to the Angiospermous flora of the Tertiary. In Europe the Cretaceous flora was apparently more like that of the Lias and Oolite than in this country, for while the genera *Salix*, *Acer*, *Populus*, *Alnus*, *Quercus*, &c., were then introduced there as here, its general aspect was modified by the presence of numbers of *Cycadaceæ*, and its sub-tropical character attested by fan-palms.

We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe; but, as far as at present known, our plants of this age present an *ensemble* quite different. I have now some sixty to seventy species of Cretaceous plants, collected in New Jersey and in various parts of the great Cretaceous area of the interior of the continent, all of which indicate a flora very similar to that now occupying the same region; many, perhaps most, of the genera being now represented in our forests—such as *Liriodendron*, *Platanus*, *Acer*, *Populus*, *Salix*, *Alnus*, *Fagus*, &c. These specimens have been collected in localities included between the 36th and 41st parallels of latitude, but range from the 74th to the 110th of longitude. Nowhere within this area have I yet detected any traces of palms or any indications of a tropical climate. At the base of the Yellow Sandstone series of New Mexico (Lower Cretaceous) I have found a varied and interesting flora, containing *Pterophyllum*, *Nilssonia*, *Camptopteris*, &c., with a few Angiosperm dicotyledonous leaves. This is evidently the point of junction between the Cycadaceous flora of the Jurassic age and that of the Chalk; for in the entire overlying Cretaceous strata, 4000 feet in thickness, though Angiospermous leaves are abundant, those of Gymnospermous plants were nowhere discovered, nor any traces of palms, either leaves or stems. The sandstones of the Cretaceous series contain immense numbers of silicified trunks, but they are for the most part coniferous.

4th, For the glimpses I have obtained of the Tertiary flora of North America I am mainly indebted to the kindness of Dr Hayden, who has spent several years in most successfully exploring the geology, botany, and zoology of the country bordering the Upper Missouri. Among his rich collections are fifty or more species of beautifully preserved fossil plants from the Miocene, which have been put in my hands for examination, and of which descriptions will be published immediately after my return to Washington.

Not having the specimens, or my notes on them, with me, I can speak only generally of the flora they represent. I remember, however, that they include species of *Platanus*,—one of which closely resembles Unger's great *P. Hercules*, and is perhaps as large; *Populus*, *Acer*, *Castanea*, *Sapindus*, *Carpinus*, *Ulmus*, *Diospyros*, *Quercus*, *Salix*, *Taxodium*, and others which indicate a flora in all its general aspects similar to that now occupying the Valley of the Mississippi. A few plants in the collection would seem to have required a somewhat warmer climate than that which the localities where they are found enjoy at present; but there are no palms among them, nor any of the tropical genera *Cinamomum*, *Sterculia*, *Dombeyopsis*, &c., so common in the Tertiary strata of Europe.

In the enumeration of the Miocene plants of the Pacific coast, given by Mr Lesquereux, I find also evidence of a marked and interesting difference of temperature during the Tertiary epoch, in different parts of the North American continent, under the same parallels of latitude. Mr Lesquereux finds in Dr Evans's collection of Palms, *Salisburia*, *Cinamomum*, &c., which indicate, at least a sub-tropical climate; a flora quite unlike that from the Miocene of the Upper Missouri, although, as he remarks, similar to that of the Miocene of Europe.

I am tempted to dwell for a moment on the interesting glimpses of the physical geography of our continent in geological times, which these facts and others that have come under my observation afford.

1st, A large continental area occupied the place of the interior of North America, from the earliest Palæozoic ages.

2d, During the Carboniferous epoch, this land sustained a vegetation similar to that of the Coal period of Europe and Eastern America, though far less varied.

3d, Through the Triassic and Jurassic ages, the sediments from the land were strikingly like, in mineral character, to those of the same age in the Old World: and the flora was characterised by a preponderance of Cycadaceous plants, analagous to those of the Jurassic of Europe.

4th, In the Cretaceous age, the central nucleus of the continent was sufficiently extensive to furnish from its ruins arenaceous sediments that now cover more than half a million square miles. These sediments contain vast deposits of carbonaceous matter, mainly derived from the land plants which covered the continent. As far south as lat. 35° these plants were for the most part Coniferous or Angiospermous, and included many genera now characteristic of temperate climates.

Through the Tertiary epoch, our continent had nearly the form and area it now has, the Tertiary deposits merely skirting its borders. The Marine Tertiaries are nearly limited to the shores of the present oceans, while the patches of strata of that age found nearer the centre of the continent are all, so far as I have observed or heard, of fresh water or estuary origin. Between the western base of the Sierra Nevada and the Mississippi there are, I believe, no Tertiary beds not of this character, and the larger part of the great central plateau has never been covered with Tertiary or Drift sediments, but has, since the close of the Cretaceous epoch, been as now, dry land.

The facts which I have enumerated seem to indicate that over this ancient land the isothermal lines were curved much as now, and that during the Tertiary ages there was perhaps as great a difference between the climate of the Pacific and Atlantic watersheds as exists at present.—*Silliman's Journal.*

CHEMISTRY.

On the Quantities of Nitrogen in the Soil at Various Depths. By M. I. Isadore Pierre, Corresponding Member of the Institute, Professor of the Faculty of Sciences of Caen.

This author lately brought before the Academy of Sciences a detail of elaborate experiments on the subject announced, which are of no small scientific and agricultural interest, and of which the following is an abstract:—

They were made on two fields in the neighbourhood of Caen, about a third of a mile distant from each other; the one (on which the first series of experiments was performed) a deep soil, composed of clay and lime, with a little silica, and which bore good crops of clover, sainfoin, and lucern; the other more rough and stony, and somewhat neglected. The nitrogen estimated is exclusive of nitrates.

First Series of Experiments.—These were made on a field of about (two hectares) five acres, which as its last crop had carried for two successive years a mixture of clover and sainfoin, and had not received directly any manure for nearly four years. About a year after it was ploughed up, eight holes were dug in different parts of it, so as to obtain average samples of its soil at different depths, and with the following results:—

The layer of soil extending down from the surface to (20 centimetres) nearly 8 inches, contained (per kilogramme 1 grammes 659 =) ·1639 per cent. of nitrogen. The second layer, extending down from (20 to 40 centimetres) nearly 8 to nearly 16 inches, contained (per kilogramme 1 grammes 157=) ·1157 per cent. of nitrogen. It contained besides, (per kilogramme 2 grammes 3 =) ·23 per cent. of silica, soluble in very dilute acids.

Calculating from these data the proportion of combined nitrogen in a given area, and assuming that the soil, which had not been turned up for a year, had a specific gravity equal to twice that of an equal volume of water, it thus appears that in the upper layer there are (per hectare 6636 kilogrammes =) 5920 lbs. per acre of nitrogen; and for the lower layer (per hectare 4628 kilogrammes =) 4119 lbs. per acre; and for both layers, or the whole to the depth of nearly 16 inches (per hectare 11,264 kilogrammes =) 10,039 lbs. per acre, and that without counting the nitrates.

Second Series of Experiments.—These were made in another field, stony and in bad condition, and with a view to ascertain the quantity of nitrogen at greater depths below the surface, namely to that of (1 metre) nearly 40 inches. And from four layers of equal thickness, descending to nearly 40 inches, there were obtained the following results for the whole of the rough soil:—

	Nitrogen.	
	Per Hectare.	Per Acre.
1. Surface layer, ...	8,366 kil.	7,464 lbs.
2. Second do.	4,959 kil.	4,415 lbs.
3. Third do.	3,479 kil.	3,097 lbs.
4. Fourth do.	2,816 kil.	2,507 lbs.
Sum,	19,620 kil.	17,413 lbs.

These results both explain the great amount of nitrogen which green crops may carry off the same land year after year, and show the value of such crops with tap roots, not only for the sake of the fodder which they yield, but for the sake of bringing up by their roots into the surface stratum, where the cereals grow, nitrogen which were otherwise inaccessible to them, and which is left there in their roots when the green crop is cut.*

Dufour on a Fluorescent Solution from Fraxinus Ornus, *the Manna Ash.*—Stokes has shown that several organic substances are capable of showing fluorescence. Recently Prince Salm-Horstman has pointed out the fluorescence caused by *fraxine* extracted from *Fraxinus excelsior* (the common ash). Dufour states that a liquid endowed with beautiful fluorescent properties may be procured by means of the manna ash (*Fraxinus ornus*). By throwing into water some pieces of bark, there are immediately produced beautiful blue reflexions, and in less than a minute there is a solution exhibiting the most beautiful fluorescence. The intensity of the effect surpasses that produced by sulphate of quinine. This solution, examined according to Stoke's methods, shows very well the characters of fluorescence, but it gives especially a marked coloration by the aid of the electrical light of Geissler. By taking one of Geissler's tubes, when the electrical current is surrounded by a liquid column, we obtain a shade of pure and intense blue. The facility and rapidity with which this solution can be obtained, without any chemical operation, and by the aid of a thin branch of *Fraxinus ornus*, render it useful for many experiments.

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* This communication is to be found at length in the "Annales de Chimie et de Physique" for May 1860, p. 63.

GEOLOGY.

Climate of Canada in the Pleistocene Period. By Principal DAWSON, Montreal.—The climate of this period, and the causes of its difference from that which now obtains in the northern hemisphere, have been fertile subjects of discussions and controversies, which I have no wish here to reopen. I merely propose to state, in a manner level to the comprehension of the ordinary reader, the facts of the case in so far as relates to Canada, and an important inference to which they appear to me to lead, and which, if sustained, will very much simplify our views of this question.

Every one knows that the means and extremes of annual temperature differ much on the opposite sides of the Atlantic. The isothermal line of 40°, for example, passes from the south side of the Gulf of St Lawrence, skirts Iceland, and reaches Europe near Drontheim in Norway. This fact, apparent as the result of observations on the temperature of the land, is equally evidenced by the inhabitants and physical phenomena of the sea. A large proportion of the shell-fish inhabiting the Gulf of St Lawrence and the coast thence to Cape Cod, occur on both sides of the Atlantic, but not in the same latitudes. The marine fauna of Cape Cod is parallel in its prevalence of boreal forms with that of the south of Norway. In like manner the descent of icebergs from the north, the freezing of bays and estuaries, the drifting and pushing of stones and boulders by ice, are witnessed on the American coast in a manner not paralleled in corresponding latitudes in Europe. It follows from this that a collection of shells from any given latitude on the coasts of Europe or America, would bear testimony to the existing difference of climate. The geologist appeals to the same kind of evidence with reference to the climate of the later tertiary period, and let us inquire what is its testimony.

The first and most general answer usually given is, that the pleistocene climate was colder than the modern. The proof of this in Western Europe is very strong. The marine fossils of this period in Britain are more like the existing fauna of Norway or of Labrador than the present fauna of Britain. Great evidences exist of driftage of boulders by ice, and traces of glaciers on the higher hills. In North America the proofs of a rigorous climate, and especially of the transport of boulders and other materials by ice, are equally good, and the marine fauna all over Canada and New England is of boreal type. In evidence of these facts I may appeal to the papers and other publications of Sir C. Lyell and Professor Ramsay, on the formations of the so-called glacial period in Europe and America,* and to my own previous papers on the tertiaries of Canada.

Admitting, however, that a rigorous climate prevailed in the Pleistocene period, it by no means follows that the change has been equally great in different localities. On the contrary, while a great and marked revolution has occurred in Europe, the evidences of such change are very much more slight in America. In short, the causes of the coldness of the Pleistocene seas to some extent still remain in America, while they must have disappeared or been modified in Europe.

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* Lyell's Travels in North America, Ramsay on the Glaciers of Wales, and on the Glacial Phenomena of Canada. See also Forbes on the Fauna and Flora of the British Islands, in "Memoirs of Geological Survey."

If we inquire as to these causes as at present existing, we find them in the distribution of ocean currents, and especially in the great warm current of the Gulf-stream, thrown across from America to Europe, and in the arctic currents bathing the coasts of America. In connection with these we have the prevailing westerly winds of the temperate zone, and the great extent of land and shallow seas in Northern America. Some of these causes are absolutely constant. Of this kind is the distribution of the winds depending on the earth's temperature and rotation. The courses of the currents are also constant, except in so far as modified by coasts and banks; and the direction of the drift-scratches and transport of boulders in the Pleistocene both of Europe and America, show that the arctic currents at least have remained unchanged. But the distribution of land and water is a variable element, since we know that at the period in question nearly all northern Europe, Asia, and America were at one time or another under the waters of the sea, and it is consequently to this cause that we must mainly look for the changes which have occurred.

Such changes of level must, as has been long since shown by Sir Charles Lyell, modify and change climate. Every diminution of the land in arctic America must tend to render its climate less severe. Every diminution of land in the temperate regions must tend to reduce the mean temperature. Every diminution of land anywhere must tend to diminish the extremes of annual temperature; and the condition of the southern hemisphere at present shows that the disappearance of the great continental masses under the water would lower the mean temperature but render the climate much less extreme. Glaciers might then exist in latitudes where now the summer heat would suffice to melt them, as Darwin has shown that in South America glaciers extend to the sea-level in latitude 46° 50′; and at the same time the ice would melt more slowly and be drifted farther to the southward. Any change that tended to divert the arctic currents from our coasts would raise the temperature of their waters. Any change that would allow the equatorial current to pursue its course through to the Pacific, or along the great inland valley of North America, would reduce the British seas to a boreal condition.

The boulder formation and its overlying fossiliferous beds prove, as I have in a previous paper endeavoured to explain with regard to Canada, and as has been shown by other geologists in the case of other regions, that the land of the northern hemisphere underwent, in the later tertiary period, a great and gradual depression, and then an equally gradual elevation. Every step of this process would bring its modifications of climate, and when the depression had attained its maximum, there probably was as little land in the temperate regions of the northern hemisphere as in the southern now. This would give a low mean temperature and an extension to the south of glaciers, more especially if at the same time a considerable arctic continent remained above the waters, as seems to be indicated by the effects of extreme marine glacial action on the rocks under the boulder clay. These conditions, actually indicated by the phenomena themselves, appear quite sufficient to account for the coldness of the seas of the period, and the wide diffusion of the Gulf-stream caused by the subsidence of American land, or its entire diversion into the Pacific basin,* would give that assimilation of the American and European climates so characteristic of the time. The climate of Western Europe, in short, would under such a state of things be greatly reduced in mean temperature, the climate of America would suffer a less reduction of its mean temperature, but would be much less extreme than at present; the general effect being the establishment of a more equable but lower temperature throughout the northern hemisphere. It is perhaps necessary to add that the existence on the land, during this period of depression, of large elephantine mammals in northern latitudes, as for instance the Mammoth and Mastodon, does not contradict this conclusion. We know that these creatures were clothed in a manner to fit them for a cool climate, and an equable rather than a high temperature was probably most conducive to their welfare, while the more extreme climate consequent on the present elevation and distribution of the land may have led to their extinction.

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* This is often excluded from consideration, owing to the fact that the

The establishment of the present distribution of land and water, giving to America its extreme climate, leaving its seas cool and throwing on the coasts of Europe the heated water of the tropics, would thus affect but slightly the marine life of the American coast, but very materially that of Europe, producing the result so often referred to in these papers, that our Canadian Pleistocene fauna differs comparatively little from that now existing in the Gulf of St Lawrence, though, in so far as any difference subsists, it is in the direction of an arctic character. The changes that have occurred are perhaps all the less, that so soon as the Laurentide hills to the north of the St Lawrence valley emerged from the sea, the coasts to the south of these hills would be effectually protected from the heavy northern ice-drifts and from the arctic currents, and would have the benefit of the full action of the summer heat—advantages which must have existed to a less extent in Western Europe.

It is farther to be observed, that such subsidence and elevation would necessarily afford great facilities for the migration of arctic marine animals, and that the difference between the modern and newer Pliocene faunas must be greatest in those localities to which the animals of temperate regions could most readily migrate after the change of temperature had occurred.

It has been fully shown by many previous writers on this subject, that the causes above referred to are sufficient to account for all the local and minor phenomena of the stratified and unstratified drifts, and for the driftage of boulders and other materials, and the erosion that accompanied its deposition. Into these subjects I do not propose to enter; my object in these remarks being merely to give the reasons for my belief stated in previous papers on this subject, that the difference of climate between Pleistocene and modern Canada, and the less amount of that difference relatively to that which has occurred in western Enrope, may be explained by a consideration of the changes of level which the structure and distribution of the boulder clay and the overlying fossiliferous beds prove to have occurred.

On a Flint Implement recently discovered at the base of some beds of Drift Gravel and Brick earth at St Acheul, near Amiens. By John Wickham Flower, Esq.—The implement or weapon, the subject of these observations, was found by me about a month since, when, in company with Mr Prestwich and other Fellows of this Society, I visited some gravel-pits near Amiens. When discovered, it was imbedded in a com-

marine fauna of the Gulf of Mexico differs almost entirely from that of the Pacific coast; but the question still remains whether this difference existed in the later tertiary period, or has been established in the modern epoch, as a consequence of changed physical conditions.

pact mass of gravel, composed of large chalk-flints much water-worn and rolled, and small chalk-pebbles. It was found lying at the depth of sixteen feet from the upper surface, and about eighteen inches from the face or outer surface of the quarry, to which extent the gravel had been removed by me before I found it. The bed of gravel in question forms the capping or summit of a slight elevation of the chalk. A section of this pit, which Mr Prestwich lately exhibited to the Royal Society,* showed that the gravel presents here a thickness of about ten feet. Above this occurs a thin bed of coarse, white, siliceous sand, interspersed with small rounded chalk-pebbles; and above the sand is a layer of strong loam, of a red colour, which is now extensively worked for the purpose of making bricks. The remains of the elephant, horse, and deer have been occasionally found in the gravel; and we found in the sand which rests upon it an abundance of land and freshwater shells, all of recent species. No fossils of any kind were discovered by us in the brick-earth lying on the surface. At the distance of a few hundred yards from the convent of St Acheul are the remains of an ancient Roman cemetery. A large stone tomb is here left standing on the surface, the brick-earth having been cleared away from it; and here many Roman coins and bronze ornaments are found.

At St Roch (about half a mile distant from St Acheul), we also examined a quarry of flint-gravel, of precisely the same character, and apparently of the same period, as that of St Acheul. We procured from it two very fine tusks of the *hippopotamus*, which had been found twenty feet from the surface. These were but little rolled or broken, and it seems probable, therefore, that the same forces that transported these flint implements to their present position may also have deposited these remains of the *hippopotamus*.

The first discovery of these flint instruments, as well in this quarry as in other localities in the Valley of the Somme, is due to M. Boucher de Perthes, of Amiens. It was with a view to verify by personal observation the result of his researches that our visit to St Acheul and the neighbourhood was undertaken. Mr Prestwich had, indeed, previously visited the spot, and had embodied the result of his researches in a paper which was read before the Royal Society in May last. He had not, however, succeeded in finding one of these implements *in situ*, although he had procured several of them from the labourers. It was only after labouring for several hours that I succeeded in disinterring the specimen in question.

The result of our examination perfectly satisfied us, as it had already satisfied Mr Prestwich, of the frequent occurrence of these weapons or implements beneath the beds of loam, sand, and gravel which I have described. We not only found two good specimens of these implements, but we brought away upwards of thirty others, taken from the same pit. Some of these were found at about the same depth as that which I discovered, and some about four feet lower down. They were procured without difficulty from the labourers and their children. Mr Prestwich, on the occasion of his first visit, in company with Mr Evans, brought away about twenty specimens; and many others are to be seen in M. Boucher de Perthes' museum. They are so common in the pit in question as to have acquired a trivial name, and are known by the workpeople as *langues de chat*.

There is one peculiarity in these implements which appears to deserve particular notice; they were evidently water-worn and rounded pebbles before they were formed into weapons or tools; and this, indeed, is just such a condition as we should expect to find. None but people destitute of iron would have been content to use such rude and uncouth instruments as these; and a people unprovided with iron would also have been unable to quarry the chalk for the sake of the flint imbedded in it, but would have been forced to content themselves with those fragments which lay scattered upon the surface, or but a little below it. If we examine the specimens closely, we find that, while the manufactured or worked surfaces (namely the cutting edges and the point) are nearly as sharp and clear as if worked yesterday, the portion left of the original, or, if we may so call it, the *natural* surface (that which has not been struck off in the course of manufacture), is often very much water-worn; and it also presents that peculiar discoloration usually found in flints long exposed to the influence of the atmosphere, extending to the depth of a quarter or an eighth of an inch, and probably due to some chemical change resulting from mechanical forces.

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* Proc. Roy. Soc., vol. x. No. 35, p. 51.

It would thus seem that those forces, whatever they may have been, by means of which these implements were carried into their present position, were in operation but for a short period, since otherwise the sharp edges which they still retain would have been rounded and worn, if not altogether obliterated; and further, that the rolled and discoloured surface of the flint-pebbles with which they are associated (and from which, indeed, it seems probable that they were originally taken and fashioned) was due to some former change—the drift or gravel having subsequently been merely shifted from some other spot, bearing these implements with it, just as the loose ballast in the hold of a vessel is shifted and rolled from one side to another.

No one who attentively examines these implements can doubt that they are the products of human skill. Rude and uncouth as they may appear, that rudeness is probably not so much due to any deficiency of intelligence in the manufacturers, as to the want of iron or some other metals wherewith to work. Probably no workman who found himself destitute of metal would be able to produce from flint-pebbles more useful or elegant implements. Those who are familiar with the forms which are presented in those flints which are casually fractured will agree that it is almost impossible that even a single flint should be so fractured by accident as to assume the shape of these implements; but here we have a great number, all taken from a single quarry. Further, it will be seen that the original or natural surface is never retained where it at all interferes with the shape and symmetry of the weapon. Wherever it would have so interfered, chiefly on the sides and at the point, it has been chipped away; and thus there has been no waste of labour, nothing having been removed but that which was inconvenient. It will also be noticed that they are all formed after a certain rude but uniform pattern; they are worked to a blunt point at one end, with a rude cutting edge on each side, and a sort of boss at the other extremity, forming a handle or hand-hold. In order the better to form this double edge, a ridge is left running down the centre; and the edges have been formed by striking away the flint in splinters from each side, in a direction at right angles with, or a little oblique to, the axis, the base or under side being usually either flat, or but slightly convex.

The discovery of these implements under the circumstances indicated cannot fail to suggest many interesting inquiries. We should all desire to know something more concerning the persons by whom, and the purposes for which, they were fabricated,—how it happened that so many of them were brought together in so small a space, and how it is that no remains have hitherto been found of those by whom they were made and used. These, however, are speculations which seem to belong to the province of archæology rather than to that of geology; and they are only now alluded to by way of suggestion that topics of such importance and interest are well deserving the investigation of archæologists.—*Quarterly Journal of the Geological Society.*

The total absence of any other traces of primæval man, than the so-called "flint-implements," in the localities where these are found, is certainly a very curious, indeed, at first sight, most perplexing fact, connected with the gravel which entombs them. It seems all the more incompatible with the belief in the human origin of the flints, awakened by the mere appearance of these bodies, inasmuch as the stratum containing them embeds numerous bones and teeth of the large extinct mammals,—*Elaphus primigenius*, *Rhinoceros tichorhinus*, *Hippopotamus major*, and others, distinctive of the Pleistocene diluvium of Europe, none of them more susceptible of preservation, we must suppose, than the bones and teeth of men.

But, assuming the flint hatchets to be valid evidence of the contemporaneous existence, in the region where they are found, of a race of men, our surprise at the non-appearance of human bones will, I conceive, be materially abated, when we recall certain analogous instances of an absolute destitution of the actual remains of extinct beings of whose past existence we nevertheless possess testimony the most incontrovertible. Wherein, I would ask, is this case more puzzling than the parallel one of the *equally entire absence* of the fossilized remains of the numerous species of birds and reptiles whose "tracks" or "foot-prints" are so plainly discernible and so multiplied, in the Triassic Red Sandstone of the valley of the Connecticut in the United States. Reflecting upon man's pre-eminent resources for escape from drowning, supplied by his higher intelligence, his exemption from a wholesale burial during certain physical catastrophes is on the whole less wonderful than the similar avoidance of it by some of the inferior animal races who occupied the earth before him.

The birds which left the Connecticut sandstone "foot-marks," possibly flew away to secure retreats whenever a sudden incursion of the waters threatened to overwhelm them; might not the men of the flint-implements have found escape equally easy to tracts inaccessible to the inundations which buried up their flint hatchets and the bones of the jungle-frequenting quadrupeds, whose natural residence would be where the waters would more promptly overtake them?

To be sure, this suggestion is not very admissible, if we hold the stratum which embeds the "wrought flints" to be the so-called Diluvium of the earlier geologists, for the flood or floods which deposited it were altogether too violent, deep, and wide-spread to be compatible with that proximity and abundance of dry land, essential to the rescue of *all* the human beings of the region. Adopting the alternative hypothesis which regards the gravel as *local*, and the product of more than one inundation, we experience much less difficulty in accounting for the preservation from entombment of the human beings who fashioned the flint tools and weapons.—(H. D. R.)

On the Occurrence of Flint Implements, associated with the Remains of Extinct Mammalia, in Undisturbed Beds of a late Geological Period. By JOSEPH PRESTWICH, Esq., F.R.S., F.G.S., &c.—The author commences by noticing how comparatively rare are the cases even of the alleged discovery of the remains of man or of his works in the various superficial drifts, notwithstanding the extent to which these deposits are worked; and of these few cases so many have been disproved, that man's non-existence on the earth until after the latest geological changes, and the extinction of the Mammoth, Tichorhine Rhinoceros, and other great mammals, had come to be considered almost in the light of an established fact. Instances, however, have from time to time occurred to throw some doubt on this view, as the well-known cases of the human bones found by Dr Schmerling in a cavern near Liege,—the remains of man, instanced by M. Marcel de Serres and others, in several caverns in France,—the flint implements in Kent's Cave,—and many more. Some uncertainty, however, has always attached to cave evidence, from the circumstance that man has often inhabited such places at a comparatively late period, and may have disturbed the original cave deposit; or, after the period of his residence, the stalagmitic floor may have been broken up by natural causes, and the remains above and below it may have thus become mixed together, and afterwards sealed up by a second floor of stalagmite. Such instances of an imbedded broken stalagmitic floor are in fact known to occur; at the same time, the author does not pretend to say that this will explain all cases of intermixture in caves, but that it lessens the value of the evidence from such sources.

The subject has, however, been latterly revived, and the evidence more carefully sifted by Dr Falconer; and his preliminary reports on the Brixham Cave,* presented last year to the Royal Society, announcing the carefully determined occurrence of worked flints mixed indiscriminately with the bones of the extinct Cave bear and the rhinoceros, attracted great and general attention amongst geologists. This remarkable discovery, and a letter written to him by Dr Falconer on the occasion of his subsequent visit to Abbeville last autumn, instigated the author to turn his attention to other ground, which, from the interest of its later geological phenomena alone, as described by M. Buteux in his "Esquisse Géologique du Departement de la Somme," he had long wished and intended to visit.

In 1849 M. Boucher de Perthes, President of the Société d'Emulation of Abbeville, published the first volume of a work entitled "Antiquités Celtiques et Antédiluviennes," in which he announced the important discovery of worked flints in beds of undisturbed sand and gravel containing the remains of extinct mammalia. Although treated from an antiquarian point of view, still the statement of the geological facts by this gentleman, with good sections by M. Ravin, is perfectly clear and consistent. Nevertheless, both in France and England, his conclusions were generally considered erroneous; nor has he since obtained such verification of the phenomena as to cause so unexpected a fact to be accepted by men of science. There have, however, been some few exceptions to the general incredulity. The late Dr Rigollot, of Amiens, urged by M. Boucher de Perthes, not only satisfied himself of the truth of the fact, but corroborated it, in 1855, by his "Mémoire sur des Instruments en Silex trouvés à St Acheul." Some few geologists suggested further inquiry; whilst Dr Falconer, himself convinced by M. de Perthes' explanations and specimens, warmly engaged Mr Prestwich to examine the sections.

The author, who confesses that he undertook the inquiry full of doubt, went last Easter, first to Amiens, where he found, as described by Dr Rigollot, the gravel-beds of St Acheul capping a low chalk-hill a mile S.E. of the city, about 100 feet above the level of the Somme, and not commanded by any higher ground. The following is the succession of the beds in descending order:—

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* On the 4th of May, this year, Dr Falconer further communicated to the Geological Society some similar facts, though singularly varied, recently discovered by him in the Maccagnone Cave near Palermo.—*See Proc. Geol. Soc.*

	Average thickness.
1. Brown brick-earth (*many old tombs and some coins*), with an irregular bed of flint-gravel. No organic remains	10 to 15 ft.
Divisional plane between 1 *and* 2a *very uneven and indented.*	
2a. Whitish marl and sand with small chalk debris. Land and fresh water shells (*Lymnea*, *Succinea*, *Helix*, *Bithynia*, *Planorbis*, *Pupa*, *Pisidium*, and *Ancylus*, all of recent species) are common, and mammalian bones and teeth are occasionally found	2 to 8 ft.
2b. Coarse subangular flint-gravel,—white with irregular ochreous and ferruginous seams,—with tertiary flint pebbles and small sandstone blocks. Remains of shells as above, in patches of sand. Teeth and bones of the elephant, and of a species of horse, ox, and deer,—generally near base. This bed is further remarkable for containing worked flints ("Haches" of M. de Perthes, and "Langues de Chat" of the workmen)	6 to 12 ft.
Uneven surface of chalk.	

The flint implements are found in considerable numbers in 2*b*. On his first visit, the author obtained several specimens from the workmen, but he was not successful in finding any himself. On his arrival, however, at Abbeville, he received a message from M. Pinsard of Amiens, to whose co-operation he expresses himself much indebted, to inform him that one had been discovered the following day, and was left *in situ* for his inspection. On returning to the spot, this time with his friend Mr Evans, he satisfied himself that it was truly *in situ*, 17 feet from the surface, in undisturbed ground, and he had a photographic sketch of the section taken.*

Dr Rigollot also mentions the occurrence in the gravel of round pieces of hard chalk, pierced through with a hole, which he considers were used as beads. The author found several, and recognised in them a small fossil sponge, the *Coscinopora globularis*, D'Orb., from the chalk, but does not feel quite satisfied about their artificial dressing. Some specimens do certainly appear as though the hole had been enlarged and completed.

The only mammalian remains the author here obtained were some specimens of the teeth of a horse, but whether recent or extinct, the specimens were too imperfect to determine; and part of the tooth of an elephant (*Elephas primigenius?*). In the gravel-pit of St Roch, 1½ mile distant, and on a lower level, mammalian remains are far more abundant, and include *Elephas primigenius*, *Rhinoceros tichorhinus*, *Cervus somonensis*, *Bos priscus*, and *Equus*;† but the workmen said that no worked flints were found there, although they are mentioned by Dr Rigollot.

At Abbeville the author was much struck with the extent and beauty of M. Boucher de Perthes' collection. There were many forms of flints, in which he, however, failed to see traces of design or work, and which he should only consider as accidental; but with regard to those flint instruments termed "axes" ("haches") by M. de Perthes, he entertains not the slightest doubt of their artificial make. They are of two forms, generally from 4 to 10 inches long. They are very rudely made, without

* On revisiting the pit, since the reading of this paper, in company with several geological friends, the author was fortunate to witness the discovery and extraction by one of them, Mr J. W. Flower, of a very perfect and fine specimen of flint implement, in a seam of ochreous gravel, 20 feet beneath the surface. They besides obtained thirty-six specimens from the workmen. —*June*, 1859.

† To this list the author has to add the *Hippopotamus*, of which creature four fine tusks were obtained on this last visit.

any ground surface, and were the work of a people probably unacquainted with the use of metals. These implements are much rarer at Abbeville than at Amiens. The author was not fortunate enough to find any specimens himself; but from the experience of M. de Perthes, and the evidence of the workmen, as well as from the condition of the specimens themselves, he is fully satisfied of the correctness of that gentleman's opinion, that they there also occur in beds of undisturbed sand and gravel.

At Moulin Quignon, and at St Gilles, to the S.E. of Abbeville, the deposit occurs, as at St Acheul, on the top of a low hill, and consists of a subangular, ochreous and ferruginous flint-gravel, with a few irregular seams of sand, 12 to 15 feet thick, reposing on an uneven surface of chalk. It contains no shells, and very few bones. M. de Perthes states that he has found fragments of the teeth of the elephant here. The worked flints and the bones occur generally in the lower part of the gravel.

In the bed of gravel also on which Abbeville stands, a number of flint implements have been found, together with several teeth of the *Elephas primigenius*, and at places fragments of fresh-water shells.

The section, however, of greatest interest is that at Menchecourt, a suburb to the N.W. of Abbeville. The deposit there is very distinct in its character; it occurs patched on the side of a chalk hill, which commands it to the northward; and it slopes down under the peat-beds of the valley of the Somme to the southward. The deposit consists, in descending order, of—

	Average thickness.
1. A mass of brown sandy clay, with angular fragments of flints and chalk rubble. No organic remains. Base very irregular and indented into bed No. 2..............	2 to 12 ft.
2. A light-coloured sandy clay ("sable gras" of the workmen), analogous to the loess, containing land shells, *Pupa*, *Helix*, *Clausilia* of recent species. Flint axes and mammalian remains are said to occur occasionally in this bed	8 to 25 ft.
3. White sand ("sable aigre"), with 1 to 2 feet of subangular flint-gravel at base. This bed abounds in land and fresh-water shells of recent species of the genera *Helix*, *Succinea*, *Cyclas*, *Pisidium*, *Valvata*, *Bithynia*, and *Planorbis*, together with the marine *Buccinum undatum*, *Cardium edule*, *Tellina solidula*, and *Purpura lapillus*. The author has also found the *Cyrena consobrina* and *Littorina rudis*. With them are associated numerous mammalian remains, and, it is said, flint implements.	2 to 6 ft.
4. Light-coloured sandy marl, in places very hard, with *Helix*, *Zonites*, *Succinea*, and *Pupa*. Not traversed......	3 +

The Mammalian remains enumerated by M. Buteux from this pit are, —*Elephas primigenius*, *Rhinoceros tichorhinus*, *Cervus somonensis* (?), *Cervus tarandus priscus*, *Ursus spelæus*, *Hyæna spelæa*, *Bos primigenius*, *Equus adamaticus*, and a *Felis*. It would be essential to determine how these fossils are distributed—which occur in bed No. 2, and which in bed No. 3. This has not hitherto been done. The few marine shells occur mixed indiscriminately with the fresh-water species, chiefly amongst the flints at the base of No. 3. They are very friable and somewhat scarce. It is on the top of this bed of flints that the greater number of bones are found, and also, it is said, the greater number of flint implements. The author, however, only saw some long flint flakes (considered by M. de Perthes as flint knives) turned out of this bed in his presence, but the workmanship was not very clear or apparent; still it was as much so as in some of the so-called flint knives from the peat-beds and barrows. There are specimens, however, of true implements ("haches") in M. de Perthes' collection, from Menchecourt; one noticed by the author was from a depth of 5, and another of 7 metres. This would take them out from bed No. 1, but would leave it uncertain whether they came from No. 2 or No. 3. From their general appearance, and traces of the matrix, the author would be disposed to place them in bed No. 2, but M. de Perthes believes them to be from No. 3; if so, it must have been in some of the subordinate clay seams occasionally intercalated in the white sand.

Besides the concurrent testimony of all the workmen at the different pits, which the author after careful examination saw no reason to doubt, the flint implements ("haches") bear upon themselves internal evidence of the truth of M. de Perthes' opinion. It is a peculiarity of fractured chalk-flints to become deeply and permanently stained and coloured, or to be left unchanged, according to the nature of the matrix in which they are imbedded. In most clay-beds they become outside of a bright opaque white, or porcelainic; in white calcareous or siliceous sand, their fractured black surfaces remain almost unchanged; whilst in beds of ochreous and ferruginous sands, the flints are stained of the light yellow and deep brown colours so well exhibited in the common ochreous gravel of the neighbourhood of London. This change is the work of very long time, and of moisture before the opening out of the beds. Now, in looking over the large series of flint implements in M. de Perthes' collection, it cannot fail to strike the most casual observer that those from Menchecourt are almost always white and bright, whilst those from Moulin Quignon have a dull yellow and brown surface; and it may be noticed that whenever (as is often the case) any of the matrix adheres to the flint, it is invariably of the same nature, texture, and colour as that of the respective beds themselves. In the same way at St Acheul, where there are beds of white and others of ochreous gravel, the flint implements exhibit corresponding variations in colour and adhering matrix; added to which, as the white gravel contains chalk debris, there are portions of the gravel in which the flints are more or less coated with a film of deposited carbonate of lime; and so it is with the flint implements which occur in those portions of the gravel. Further, the surface of many specimens is covered with fine dendritic markings. Some few implements also show, like the fractured flints, traces of wear, their sharp edges being blunted. In fact, the flint implements form just as much a constituent part of the gravel itself,—exhibiting the action of the same later influences and in the same force and degree,—as the rough mass of flint fragments with which they are associated.

With regard to the geological age of these beds, the author refers them to those usually designated as Post-pliocene, and notices their agreement with many beds of that age in England. The Menchecourt deposit much resembles that of Fisherton near Salisbury; the gravel of St Acheul is like some on the Sussex coast; and that of Moulin Quignon resembles the gravel at East Croydon, Wandsworth Common, and many places near London. The author even sees reason, from the general physical phenomena, to question whether the beds of St Acheul and Moulin Quignon may not possibly be of an age one stage older than those of Menchecourt and St Roch; but before that point can be determined, a more extended knowledge of all the organic remains of the several deposits is indispensable.

The author next proceeds to inquire into the causes which led to the rejection of this and the cases before mentioned, and shows that in the case of M. de Perthes' discovery, it was in a great degree the small size and indifferent execution of the figures, and the introduction of many forms about which there might reasonably be a difference of opinion;—in the case of the arrow-heads in Kent's Cave a hidden error was merely suspected;—and in the case of the Liege cavern, he considers that the question was discussed on a false issue. He therefore is of opinion that these and many similar cases require reconsideration; and that not only may some of these prove true, but that many others, kept back by doubt or supposed error, will be forthcoming.

One very remarkable instance has already been brought under the author's notice by Mr Evans since their return from France. In the 13th volume of the "Archæologia," published in 1800, is a paper by Mr John Frere, F.R.S. and F.S.A., entitled "An Account of Flint Weapons discovered at Hoxne in Suffolk," wherein that gentleman gives a section of a brick-pit in which numerous flint implements had been found, at a depth of 11 feet, in a bed of gravel containing bones of some unknown animal; and concludes from the ground being undisturbed and above the valley, that the specimens must be of very great antiquity, and anterior to the last changes of the surface of the country,—a very remarkable announcement, hitherto overlooked.

The author at once proceeded in search of this interesting locality, and found a section now exposed to consist of—

	Feet.
1. Earth and a few flints	2
2. Brown brick-earth, a carbonaceous seam in middle and one of gravel at base; no organic remains. The workmen stated that two flint implements (one of which they shortly picked up in the author's presence) had been found about 10 feet from the surface during last winter	12
3. Grey clay, in places carbonaceous and in others sandy, with recent land and fresh-water shells (*Planorbis, Valvata, Succinea, Pisidium, Helix,* and *Cyclas*) and bones of Mammalia	4
4. Small subangular flint-gravel and chalk pebbles	2½
5. Carbonaceous clay (stopped by water)	½+

The weapons referred to by Mr Frere are described by him as being found abundantly in bed No. 4; but at the spot where the work has now arrived, this bed is much thinner, and is not worked. In the small trench which the author caused to be dug, he found no remains either of weapons or of bones. He saw, however, in the collection of Mr T. E. Amyot, of Diss, specimens of the weapons, also an astragalus of the elephant from, it was supposed, this bed, and from bed No. 3 the teeth of a horse, closely resembling those from the elephant-bed of Brighton.

The specimens of the weapons figured by Mr Frere, and those now in the British Museum and elsewhere, present a singular similarity in work and shape to the more pointed forms from St Acheul.

One very important fact connected with this section is, that it shows the relative age of the bone and implement-bearing beds. They form a thin lacustrine deposit, which seems to be superimposed on the boulder clay, and to pass under a bed of the ochreous sand and flint-gravel belonging to the great and latest drift-beds of the district.

The author purposely abstains for the present from all theoretical considerations, confining himself to the corroboration of the facts:—

1. That the flint implements are the work of man.
2. That they were found in undisturbed ground.
3. That they are associated with the remains of extinct mammalia.
4. That the period was a late geological one, and anterior to the surface assuming its present outline, so far as some of its minor features are concerned.

He does not, however, consider that the facts, as they at present stand, of necessity carry back man in past time more than they bring forward the great extinct mammals towards our own time, the evidence having reference only to relative and not to absolute time; and he is of opinion that many of the later geological changes may have been sudden or of shorter duration than generally considered. In fact, from the evidence here exhibited, and from all that he knows regarding drift phenomena generally, the author sees no reason against the conclusion that this period of man and the extinct mammals—supposing their contemporaneity to be proved—was brought to a sudden end by a temporary inundation of the land; on the contrary, he sees much to support such a view on purely geological consideration.

The paper concludes with a letter from Mr John Evans, F.S.A. and F.G.S., regarding these implements from an antiquarian rather than a geological point of view, and dividing them into three classes:—

1. Flint flakes—arrow-heads or knives.
2. Pointed weapons truncated at one end, and probably lance or spear heads.
3. Oval or almond-shaped implements with a cutting edge all round, possibly used as sling-stones or as axes.

Mr Evans points out, that in form and workmanship those of the two last classes differed essentially from the implements of the so-called Celtic period, which are usually more or less ground and polished, and cut at the wide and not the narrow end; and that had they been found under any circumstances, they must have been regarded as the work of some other race than the Celts, or known aboriginal tribes. He fully concurs with Mr Prestwich, that the beds of drift in which they were found were entirely undisturbed.

MISCELLANEOUS.

Origin of Species.—Dr Daubeny, in speaking of Mr Darwin's work, says—"Even the most devoted admirers of Mr Darwin's work must, I think, admit thus much, that there is one link defective in the chain of his evidence. It will be observed, that the foundation of all his reasonings, the class of facts to which he can alone appeal with perfect confidence in support of his theory, are those of domestication. All the rest, however appropriate to the development of his argument, however well calculated to remove objections, or to impart a degree of probability to his speculations, seem either to lie beyond the range of actual experience, or to lend him only that indirect support which may be afforded by their accordance with the hypothesis, once assumed to be true. If, on the other hand, it could have been shown, that man effects in all respects, except as to degree, in a short time, what nature is assumed to have done in one of indefinite duration, the argument must be admitted to be complete and triumphant. But although human ingenuity has doubtless introduced many very striking deviations, both in plants and animals, from the original type, it has never yet, I believe, proceeded so far as to give rise to what naturalists would regard as a new species; that is, an individual incapable of producing a fertile progeny with any other member of the parent stock. To assert, therefore, that nature has accomplished this in the course of a vastly more extended period, although it may appear to some a fair presumption, cannot be regarded as a strictly legitimate inference, especially when it may be met by other antagonistic facts, which might lead us to believe, what nothing in Mr Darwin's work is able to contradict, namely, that nature has provided against the confusion of species by assigning certain limits to their aberrant tendencies.

What these limits may be, how far the principle of natural selection may be allowed to have extended the sphere of its operations, and to what degree, therefore, the principle so ingeniously put forward in Mr Darwin's work may justify us in reducing the now overwhelming array of species which has been set up in either kingdom of creation, it will, I believe, be the main business of naturalists for many years to come, if possible, to determine; not only directly, by noting what art has effected in the way of modifying existing races, but also indirectly, by showing what deviations from the usual condition are brought about by nature under circumstances over which we have no control."

On Botanical and Zoological Nomenclature. By WM. STIMPSON.—A more careful attention to the subject of nomenclature is urgently demanded of the followers of all branches of natural history. It is a subject to which too little attention has been paid in an abstract or general sense, and too much perhaps in particular cases. A comprehensive code of rules, recognised by the authority of the greater lights of science, has been always needed. This was attempted during the last century by Linnæus and Illiger, and in 1842 *Rules of Nomenclature* were drawn up by the British Association, and ratified by the American Association in 1845. These are excellent as far as they go, but need much extension and many additions, as any one may observe who attempts to decide by them all questions which occur in his experience.

On the other hand, in particular cases of species and genera, the discussion of questions of nomenclature has reached such a pitch, that it is no uncommon thing to see the greater part of a new zoological work devoted to synonymy. One author, after six pages of historical and synonymical matter, evincing great critical acumen and much bibliographical research, will arrive at what appears to him to be a certain and final conclusion, that the true *Orthonymus aliquis* is such and such a species. The next writer who succeeds him in the same field will triumphantly prove in *ten* pages that it is not that species at all, but the *O. neminis.* And so on to the end of the chapter, if it ever will have an end, which is doubtful, unless some decided action is soon taken by naturalists for the purging of their favourite science from this opprobrium. After all the pages which have been written upon some of these cases, we seem no nearer to a settlement than at first. The difficulty increases rather than diminishes, each succeeding author putting forth views differing from those of his predecessors. All this discussion, let us bear in mind, is merely preliminary, and for the purpose of indicating with certainty an object about which the author has perhaps not a dozen words to say.

Now it may appear at the first glance that the application of the law of priority is exceedingly simple. The name given by the first describer of a genus or species is to be respected, and applied to that genus or species throughout all time. But as soon as we come to apply this rule, we find cases without number in which complications occur, rendering limitations of the law necessary. Genera are to be subdivided, and are subdivided with different limits by different authors; the species of one are found by another to include two or three distinct forms, and so on. Some of the limitations of the law of priority have been laid down in the *Rules* of the British Association, but not enough to enable us to decide half the cases which may arise, leaving the remainder subject to the whims, or dependent upon the extent of the knowledge, of the author who would follow them.

In applying the great law, the most difficult question of all immediately arises, What constitutes a description? or, When has an author so designated his species that his name for it should hold? On this subject we have every variety of opinion, from that of the German ornithologists, who consider that a simple published name, referring to a specimen in a museum, is sufficient, to that of the lamented Edward Forbes, who once insisted that no name proposed should be accepted unless accompanied by a *Latin* description or an illustrative figure. The first opinion we believe to be scouted by nine-tenths of living naturalists; the second appears to be too stringent, as an author can of course write better in his own language than in any other, though we doubt if a description appearing in Chinese would gain the least notice from modern naturalists.

The question, "What constitutes a description?" can never be decidedly answered. No rule can be proposed which is universally applicable. With regard to its *length*;—we may say that two words are not sufficient, an hundred are; but where shall we draw the line? The two sentences of one author may be better than the two pages of another. One writer will describe an object well except in one point, in which, from defective observation, a character is represented in exact opposition to the true state of the case. Some descriptions are sufficient to enable the naturalists of one country, from their collateral knowledge, to determine a species, while those of another country or continent would be left entirely in the dark. An author may publish descriptions in a work for private distribution, which will be inaccessible to the great body of naturalists. We might fill many pages with such cases as these, and yet, were rules made out applicable to each, there would still be cases constantly arising which could be decided by none of them. How then can the matter be settled in these latter instances? We will suggest a method further on.

It will be observed that it is among the more common and earliest described species that the synonymic heap is greatest. This is exceedingly embarrassing to the student, who in general has occasion to use these very species, being those most easily accessible, in the course of his studies. He may find in a dozen different books the characters, anatomical or otherwise, of what appear to him a dozen different objects, since the names used may be different, and elementary works cannot be expected to go into synonymical details. At the present day, thanks to the advance of knowledge and precision, and the international exchange of scientific works, the name of an entirely new genus or species may escape the burden to which that of older species is subjected. It is with those published in the last century that the greatest trouble occurs. Investigators among antique and forgotten books are constantly finding some obscure work or paper, perhaps scarcely known out of its immediate vicinity even at the time it was published, in which names occur which must be adopted, in the opinion of some, to the exclusion of the familiar titles which have been used for half a century. The disinterment of Klein's name *Cyclas* is an instance of this. How strange it must seem to a conchologist of the present day to be obliged to designate the common marine *Lucina* by a name which has been in use seventy years for a fresh-water bivalve, while this fresh-water bivalve becomes *Sphærium*; and to use *Cyclostoma* for *Delphinula*, *Terebellum* for *Turritella*, &c. The restoration by G. R. Gray of Boddaert's names in ornithology is another instance. By the discovery of a meagre pamphlet of the eighteenth century, only two or three copies of which now exist, we find ourselves forced to change the generic names of common birds, familiar as they are by long and constant usage.

In the discussion of these questions all personal considerations should be entirely rejected. The smallest interest or convenience to the science in general, followed as it is by a republic of thousands, is of more importance than any compliment to the feelings of a living, or the memory of a deceased naturalist. In fact our mere recognition of an author's names is not of such vast importance to his reputation. His fame must rest upon a securer foundation than this. For the custom of placing the name of an author after a species described by him is not (or should not be) done for that author's personal advantage, but simply to assist us in the recognition of that species. It is a short method of referring to the place where the description of the species may be found, or enables us to distinguish it from some other to which the same name has been by mistake applied; as, *Pleurotoma violacea*, Hinds, *non* Mighels. In this view, how ludicrous it appears, to hear, as we often do, naturalists complain that if the custom of placing after a species the name of that author who first placed it in its proper genus is adhered to, more than one-half of Linné's species will be wrested from him. Does the fame of the great Linnæus depend upon the number of species he described?

We will now mention a few points concerning which great difference of opinion exists in the minds of naturalists, and which, for the good of science, should be immediately settled in one way or the other. The first is: shall the same generic name be allowed to occur in different departments of zoology or botany, or even in both these, or, we may add, in other sciences? Many are of the opinion that they may be used, and should not be changed, if so occurring; in view of the great difficulty now experienced in selecting a name which is not preoccupied, and shall be at the same time descriptive or suggestive of the object intended. But what is the object of a name? Surely, the *main* object is to enable us to distinguish one thing from another, and from all others, that when it is used we may know what is intended, and not be forced to decide by other aids. Is it not of vastly more importance that a name should serve this purpose, than that it should remotely indicate (which is the most generally possible) some character of the object, which it may after all hold in common with a hundred others? Greek compounds are by no means exhausted yet; and if they were, we might fall back upon euphonic names, which serve the purpose, however barbarous they may appear in the eyes of some. The custom of using the same name for many diverse objects is productive of serious inconveniences. If we have stars, countries, minerals, plants, vertebrates, articulates, mollusks and radiates, all named alike, some singular anomalies might occur, since we can of course reduplicate *specific* appellations as often as we please in different genera. For instance, suppose a travelling naturalist "making his researches in Arizona, observed specimens of the *Arizona pætula* (hermit-crab) inhabiting the shell of *Arizona pætula* (univalve), creeping among the roots of *Arizona pætula* (shrub); and upon examining it anatomically, found great numbers of the *Arizona pætula* (infusorium) living in its gills. The *Arizona pætula* (bird) was feeding upon these crabs with great voracity," &c.

Another point. A genus may contain a vast number of species, and yet, from want of profound investigations, no one may see the propriety of dividing it up. As occurs very commonly, in the course of time some new species belonging to it are described under names which, being preoccupied in that genus, are very properly changed. The new designations become established, and may be used for years. At last it becomes necessary to divide the genus, and the species whose names have been referred to are found to belong to different genera. Shall the old reduplicated specific name or the substituted one be now adhered to? Naturalists are about equally divided in opinion upon this point.

The propriety of using small initial letters to proper specific names, nouns or adjectives, has been made the subject of discussion. Whatever method be followed here, it would seem that uniformity is desirable; if any of these proper names are to have small initials, why not all? Most zoologists and botanists seem in this matter to follow the usage of their own language rather than that of the Latin, or any uniform system. The Germans will have all nouns begin with a capital, and all adjectives with a small letter, as *Ocypode Cursor*, *Chiton emersonianus*, whereas the English write common nouns with a small initial, and all *proper* appellations, whether nouns or adjectives, with a capital, as *Ocypode cursor*, *Chiton Emersonianus*. The truly convenient system will be to write all specific names, without exception, with a small initial letter, as is done by one of the most eminent zoologists of this country, and by many of those of Europe. We shall then have no difficulty in distinguishing specific from generic names, and may discuss the relations of species without the necessity of repeating the generic name or its initial every time they are mentioned. A proper name, modified for use as a specific appellation, becomes a part of a new title, and involves a different idea.—*Silliman's American Journal.*

Vegetable Parchment.—Papyrine.—The interesting substance obtained in 1846 by Poumarède and L. Figuier (Comptes Rendus, xxiii. 918; see also this Journal, xxviii. 431) by immersing bibulous paper in partially diluted sulphuric acid—called *papyrine* by its discoverers—which, with the exception of a few comparatively unimportant applications in France, where it was used for the shelves on which silk-worms are reared, &c., had excited scarcely any interest other than that naturally attaching to it as a chemical curiosity, until patented (Dec. 6, 1853) in England, by Gaine (see *Rep. of Pat. Inv.* [E. S.] xxiv. 151), and manufactured by the well-known house of De La Rue and Co., of London, has recently been investigated by Prof. A. W. Hoffmann (Ann. Ch. u. Pharm., Nov. 1859, cxii. 243; from a report to Messrs Thos. De La Rue and Co.) In its prominent properties it resembles ordinary parchment very closely: indeed the two can hardly be distinguished from each other except on close inspection. Both exhibit the same peculiar pale, yellowish tint, the same degree of translucency, the same half fibrous, horn-like texture. Like animal parchment, the artificial product is not easily torn: it may be repeatedly bent or folded without exhibiting any special appearance of breaking in the creases formed. Like ordinary parchment it is extremely hygroscopic, and becomes more pliable by absorbing moisture. When wet with water it comports itself like untanned skins, swelling up to a slippery mass through which water cannot pass except by endosmose: the coherence of the substance is not all impaired by thus soaking.

Vegetable parchment is best prepared by immersing unsized paper during a few seconds in oil-of-vitriol which has been diluted with half its volume of water, and immediately afterwards washing it in a dilute solution of ammonia; a thorough washing with pure water completing the process. Hoffman has ascertained by direct experiment that not less than one-fourth volume, or more than one-half volume, of water must be used with one volume of monohydrated sulphuric acid, in preparing the acid bath. The paper must not be immersed too long, nor should the temperature of the bath be higher than about 15° (C.)=[59° F.] A considerable amount of practice is moreover requisite before one can obtain a perfectly satisfactory product. When paper is transformed into vegetable parchment, it undergoes no appreciable increase in weight. The action of the sulphuric acid is purely molecular, the ultimate chemical composition of the paper—*cellulose*—remaining unchanged. [As already stated by Poumarède and Figuier *loc. cit.*, and by J. Barlow, Proc. of the Royal Inst. 1857, ii. 411]. The result of the momentary action of sulphuric acid in this instance is comparable with that which a longer action of this acid upon woody fibre produces—viz., formation of dextrine, a substance well known to be isomeric with cellulose. Indeed, the vegetable parchment may be regarded as a middle term between dextrine and cellulose.

The samples of parchment-paper examined by Hoffmann [and by Barlow] contained no trace of free sulphuric acid; small portions of sulphate of lime and of sulphate of ammonia being the only soluble impurities present.

There is no apparent reason why the parchment-paper should not endure for an indefinite length of time. It is evident that if its destruction were dependent in any way upon the chemicals used in preparing it, decomposition would set in at once. Nothing of the kind occurs, however; specimens of the factitious parchment which have been in Hoffmann's possession during four years being undistinguishable from those recently prepared.

From experiments made in order to ascertain the strength of parchment-paper as compared with that of true parchment and of unsized paper, it appeared that while strips of unsized paper broke when subjected to a weight of 15 or 16 lbs., several strips of vegetable parchment supported 74 lbs., and those of ordinary parchment 75 lbs., before breaking. The cohesive force of unsized paper is thus increased fivefold by the treatment with sulphuric acid. It was also proved by experiment that for equal *weights* of the two substances, parchment-paper exhibited about three-fourths the cohesive power of animal parchment. It also appeared that while the strength of strips of parchment-paper taken from different sheets was nearly constant, that of strips of animal parchment, even when cut from a single piece, was extremely variable, owing to the differences in thickness to which it is liable.

Parchment-paper, although not quite so strong as ordinary parchment, is nevertheless more capable than the latter of withstanding the action of chemical agents, and especially of resisting the action of water; it may be left in this liquid for days, or even boiled in it, without undergoing any change, other than the increase of volume already alluded to, its original cohesion, and indeed all its properties, being regained on drying. As is well known, animal parchment is soon converted into glue when boiled with water.

Since the parchment-paper contains no nitrogen, it is much less liable than ordinary parchment to putrefy when exposed to moisture, and will probably be less subject to the attacks of insects. Not only may the new parchment be substitued for that ordinarily employed for legal documents, &c.; but from its cheapness it will probably soon be used for ledgers and other important records—possibly for bank-notes, instead of the more perishable paper now employed. Its strength and power of resisting the action of moisture seem also specially to adapt it for the use of architects and engineers—particularly for working plans liable to receive rough usage; also for the envelopes of letters and for cartridges. In thin leaves it affords an admirable tracing paper. As a material for binding books, it will without doubt be extensively used. The ease with which it receives both printers' and ordinary writing ink is remarkable. For chemical laboratories it affords a most convenient material for fitting together retorts, condensers, and the like; while its power of resisting the fluids used in galvanic batteries suggests that it may be useful for diaphragms, &c. It is already used by tons, instead of bladder, as a covering for jars containing preserves, marmalades, &c.—*Silliman's American Journal.*

Professor WILLIAM B. ROGERS *on the Registering Thermometer of Dr James Lewis, of Mohawk, N. Y.*—The part of the instrument forming the thermometer proper consists of a cylindrical bundle of iron and brass wires (No. 13), about fifteen inches in length, so arranged as to be equivalent to about forty-five inches of iron wire antagonised by about an equal length of brass wire. The bundle is composed of five pairs, two of brass and three of iron, arranged alternately around the centre, and a single wire of brass, equivalent in action to a third pair of that metal, placed in the axis of the cylinder.

The upper end of the central wire, moved by the difference of expansion of the two metals, operates upon the short arm of the first of a train of two levers, and through them upon the axle of a pulley. To the grooved circumference of the larger wheel of this pulley is attached a slender silk cord carrying the *registering point* designed to mark the temperature, and which, by the multiplying effect of the mechanism, is moved over a space three hundred and twenty times as great as the differential expansion or contraction of the wires.

The registering point, properly balanced by an attached weight, and guided in its vertical movements by two slender parallel rods, is made to record the temperature on a fillet of paper moved by a train of cylinders whose axes are parallel to the guide wires. The record is impressed by the impulse of a hammer striking upon the back of the registering point at regulated intervals, and thus producing a series of small perforations in the paper, the hammer and the fillet of paper both receiving their motion from a train of clock-work of peculiar construction connected with the apparatus.

The projecting shaft of the pulley carries an index, which, revolving in front of a dial-plate placed over the pulley, enables the observer to note the temperature as compared with the ordinary thermometer, and to adjust the rod-thermometer to the standard whenever necessary. The adjustment is made by turning a screw connected with the lower end of the central brass wire of the thermometer. The latter instrument is on the outside of the case which encloses the dial, registering apparatus, and clock. By a peculiar arrangement of the clock-work, the hammer movements, and therefore the times of registration, may be adjusted to quarter-hour, half-hour, or hour intervals, and may be changed from one to the other at the will of the observer.

As regards the performance of this very ingenious instrument, Professor Rogers had obtained many interesting facts from Dr Lewis, illustrating its great sensitiveness as compared with the common mercurial thermometer, and showing the comparative steadiness and accuracy of its registration within the small limits of error due to the friction and thermal disturbances to which it is exposed. The inventor, with laudable disinterestedness, asked for a thorough scrutiny of the practical value of his contrivance. While offering information to others, he was himself a severe critic of its daily workings, and has been led since its first construction to introduce various modifications adapted to reduce resistance, to exclude radiation, and otherwise to improve its fidelity in appreciating and registering the changes of temperature. A somewhat longer experience may be needed to discover all the peculiarities of action incident to the construction of the instrument, and to give it the permanent reliability for minute registration at which the inventor aims.

Looking to the general principle of the instrument, and to the improvements thus far made in it, and relying on the faithful observation, as well as the ingenuity of Dr Lewis, for giving it all the accuracy and permanent reliability of which it is capable, Professor Rogers felt it his duty to commend the registering thermometer of Dr Lewis to the Society as an instrument worthy the critical examination of men of science, and one which promised to become a valuable help in meteorological observation.

PUBLICATIONS RECEIVED.

Quarterly Journal of the Chemical Society, No. 49.—*From the Society.*

The Glaciers of the Alps. By JOHN TYNDALL, F.R.S.—*From the Publisher.*

Canadian Naturalist and Geologist for June and August 1860.—*From the Editors.*

Journal of the Asiatic Society of Bengal, No. 1 for 1860.—*From the Society.*

Shadow-Path thrown by the Total Eclipse of the Sun on 18th July 1860 across the North-eastern part of Spain. By CHARLES VIGNOLES.—*From the Author.*

Jahrbuch der Kaiserlich-Koniglichen Geologischen Reichsanstalt, for October, November, and December 1859.—*From the Society.*

Natural History Review, for July 1860.—*From the Editors.*

An Address to the Graduates in Medicine at the Conferring of Degrees in the University of Edinburgh in August 1860. By Dr J. H. BENNETT.—*From the Author.*

Reply to Professor Tyndall's Remarks in his Work on the Glaciers of the Alps, relating to Rendu's "Théorie des Glaciers." By Principal FORBES.—*From the Author.*

Transactions of the Royal Society of Arts and Sciences of Mauritius. New Series, Vol. I., Part II.—*From the Society.*

On the Invention of Stereoscopic Glasses for Single Pictures. By T. WHARTON JONES, F.R.S.—*From the Author.*.

Address delivered to the Members of the Victoria Institute, Melbourne. By Dr FERDINAND MUELLER, President, 1860.—*From the President.*

ERRATUM.

Page 272, line 8 from the bottom, for *Senicio squalida*, read *Senecio squalidus.*

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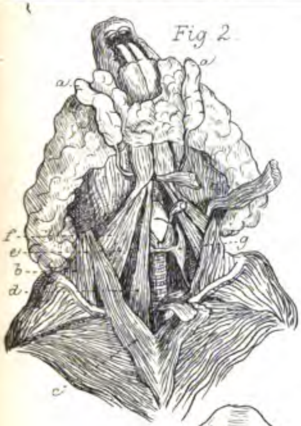
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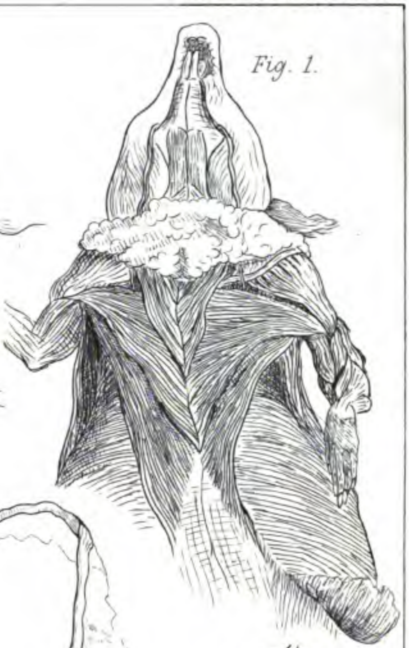
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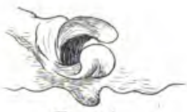
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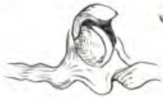
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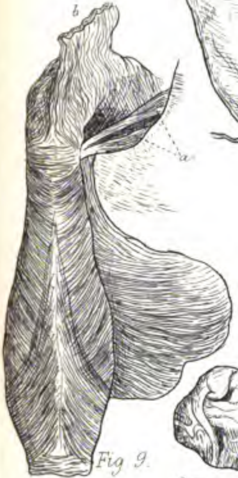
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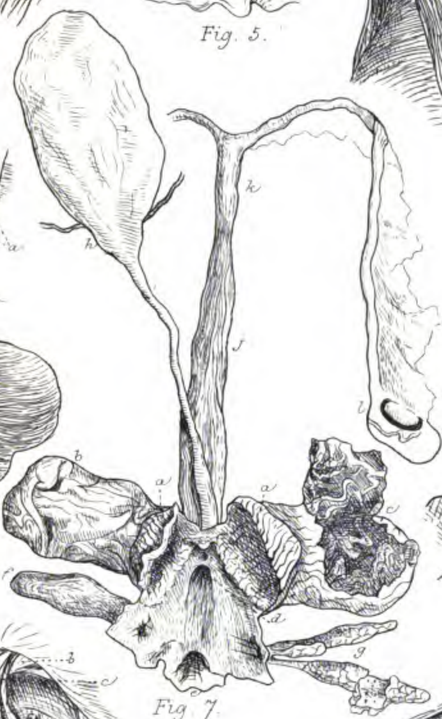
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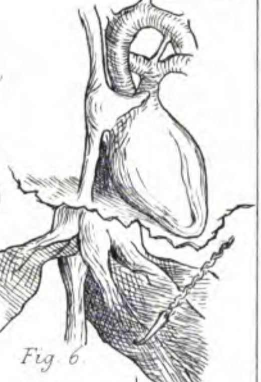
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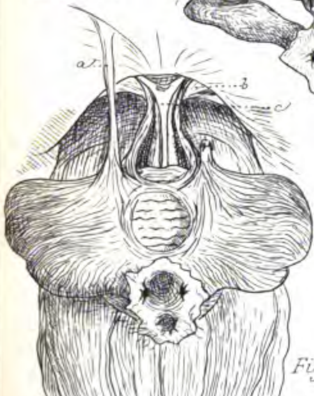
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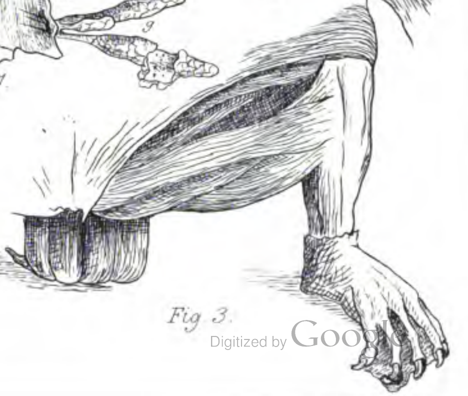
Fig. 3.

J. Cleland, delᵗ

W. H. McFarlane, Lithʳ

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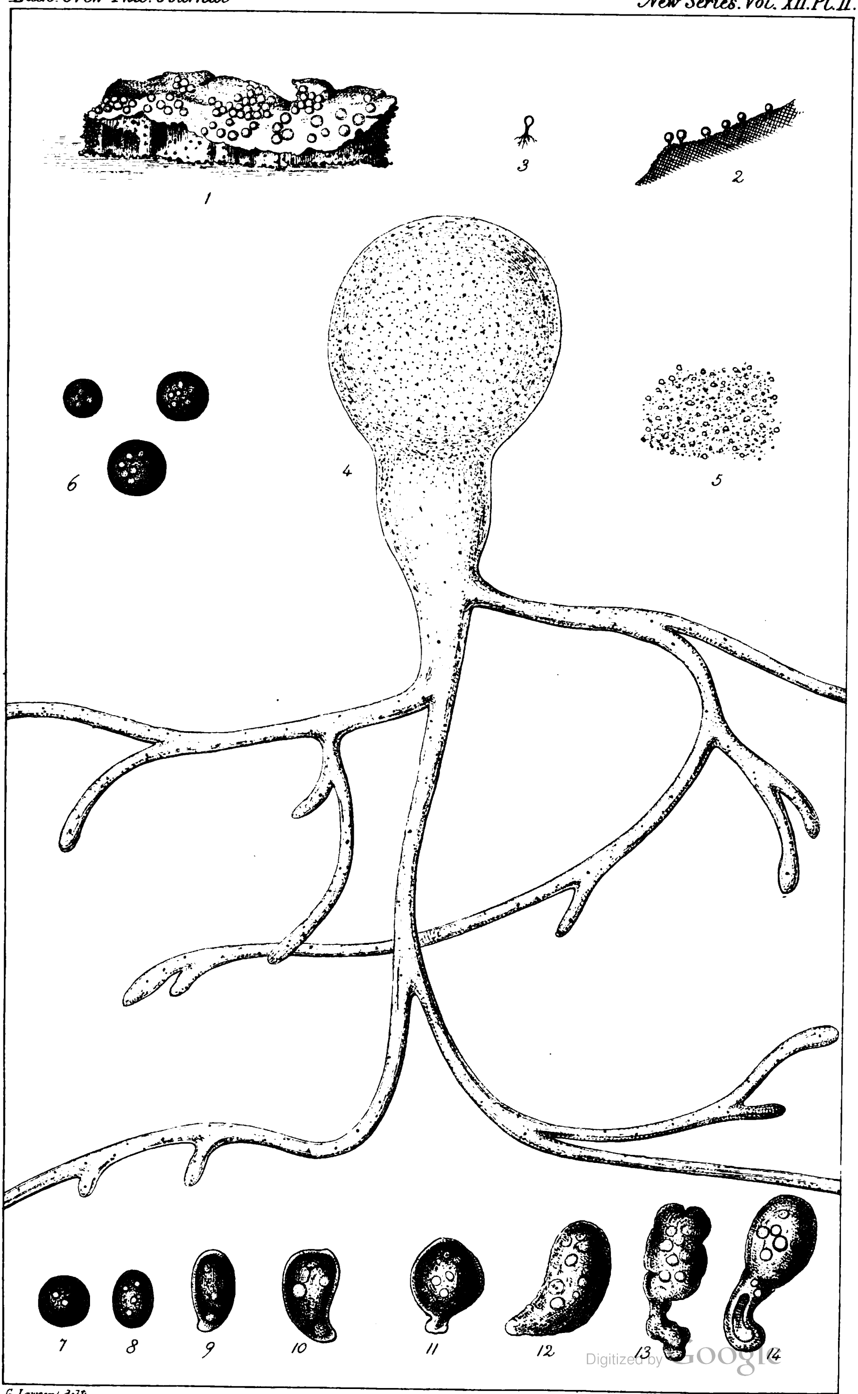
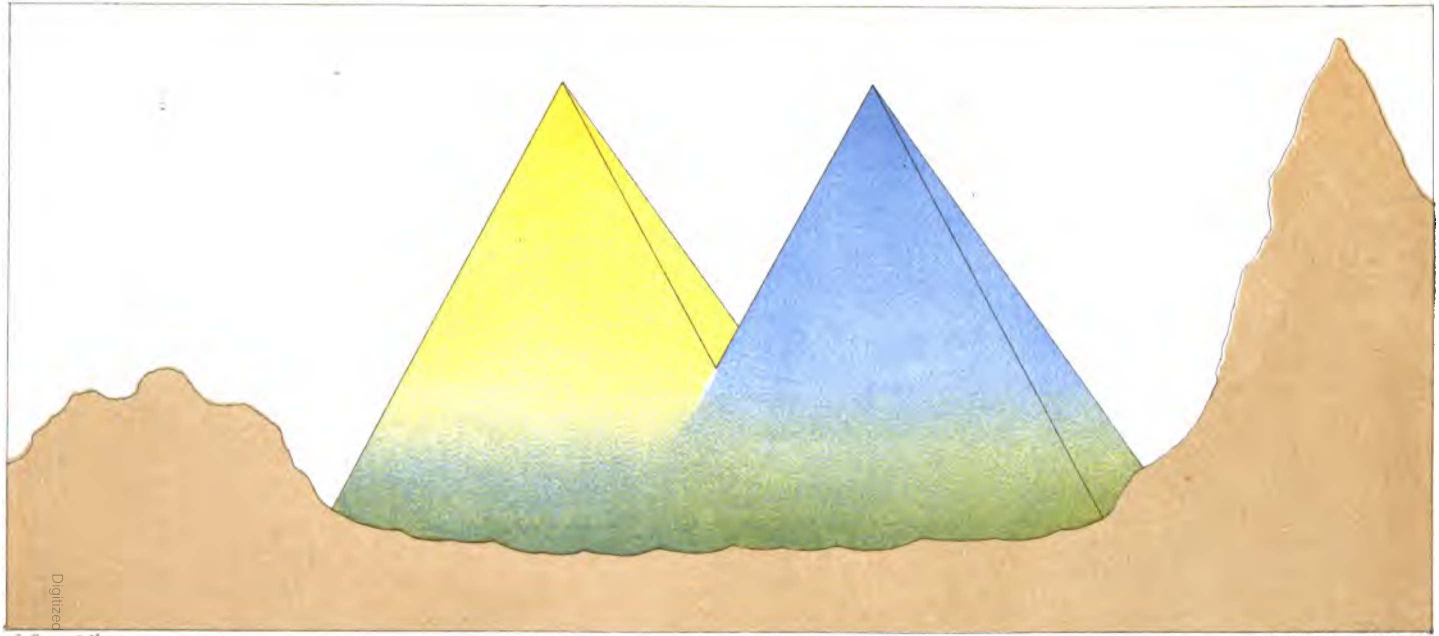


G Lawson del^t.

W. H. M^c.Farlane, Lith^r. Edin^r.



J. Hogg, del^t.

W. H. M^c. Farlane, Lith^r Edin^r.

DIAGRAM
of
NATURAL BODIES,
or of
The Four Kingdoms of Nature.

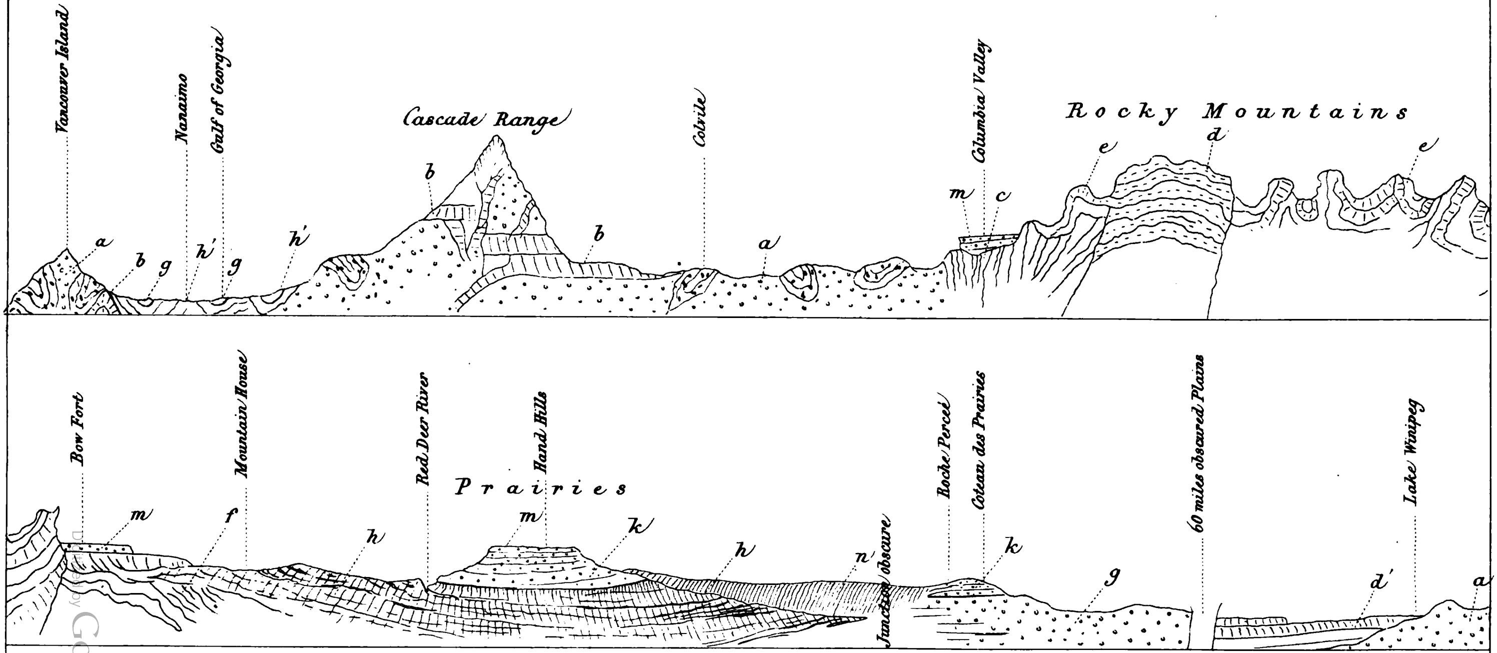
☐ MINERAL.

☐ VEGETABLE.

ANIMAL ■

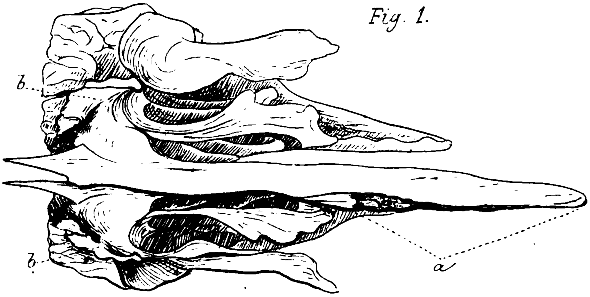
PRIMIGENAL ■

GEOLOGICAL SECTION FROM LAKE WINIPEG TO VANCOUVER ISLAND.



a. Granite & Metamorphic Rocks. - b. Basalts of Cascade Range. - c. Slates. - d. Silurian (?). - d'. Lower Silurian Limestone. - e. Carboniferous. - f. Grits and Shales (?). g. Baculite Septaria Clays Cret. - h. Lignite Group. - h'. Lignite Group of Vancouver Island. - k. Tertiary bed with brown Coal and Silicified Wood. - m. Tertiary Shingle beds of Highest Plains and Base of Mountains. - n. Tertiaries of Low Hollows.

W. H. McFarlane, Lithr. Edinr.



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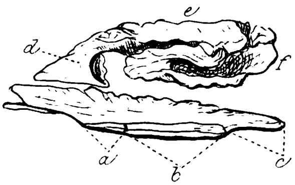

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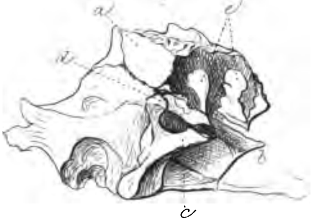

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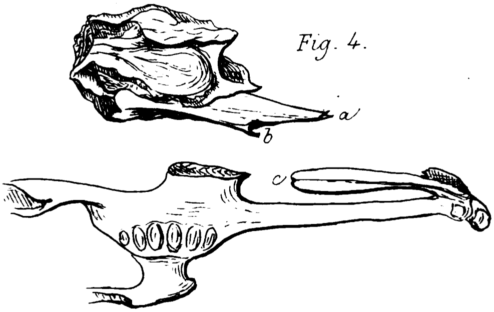

Fig. 4.

J Cleland etched on Stone.

W H. McFarlane, Lith^r Edin^r